Rapid responses to:

PAPERS:

Environmental tobacco smoke and tobacco related mortality in a prospective study of Californians, 1960-98

James E Enstrom and Geoffrey C Kabat

BMJ 2003; 326: 1057-0 [Abstract] [Full text]

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- **Re: Irresponsible journalism**

Need for clarification on competing interest 15 May 2003

Martin McKee,
Professor of European Public Health
London School of Hygiene and Tropical Medicine, London WC1E 7HT

Send response to journal:
Re: Need for clarification on competing interest

Email Martin McKee:
mailto:martin.mckee@lshtm.ac.uk?subject=Re:+Need+for+clarification+on+competing+interest

Dear editor,

Given the well documented efforts by the tobacco industry to create confusion about the link between passive smoking and disease,[1] it is essential that researchers working on this topic are seen to be entirely impartial. In this context, there are several aspects of Enstrom and Kabat's declarations on funding that require some clarification. It is true that the Center for Indoor Air Research (CIAR) is an agency receiving money primarily from US tobacco companies but this rather understates its role. As Barnes and Bero [2] have shown, in a detailed analysis of industry documents, CIAR funded two types of research, peer-reviewed and "special-reviewed", with the latter awarded directly by tobacco industry executives. Barnes and Bero showed that special-reviewed projects were more likely than peer-reviewed projects to support the tobacco industry position and be used by the industry to argue against smoking bans in public places. We should be told which category this research fell into.

In the light of experience in another case involving CIAR funded research, the editor may wish to require full documentary disclosure of the process involved.[3]

Dr Kabat may also wish to clarify some other matters. He states that he never received tobacco industry funding until last year, when he received support from a law firm that has several tobacco companies as clients.

First, given what is known of the central role played in the tobacco industry's campaign by certain lawyers, we should be told which firm this is and, specifically, whether it is one that has been linked to the industry campaign and if so, what role it played in it. Second, he published a paper in 1997 [4] with co-authors who have played leading roles in the industry campaign, one of whom was recently found by a Swiss court to have been "secretly employed by Philip Morris" as a highly paid consultant, undertaking work the court considered appeared fraudulent.[5] Consequently it will be important to have details of the nature of this earlier collaboration.

In these circumstances, the comment by Davey Smith in the accompanying editorial that the authors "may overemphasise the negative nature of their findings" cannot simply be dismissed as a genuine difference of interpretation without much more detailed scrutiny.

1 . Hong MK, Bero LA. How the tobacco industry responded to an influential study of the health effects of secondhand smoke. BMJ. 2002 Dec 14;325(7377):1413-6.

2 . Barnes DE, Bero LA. Industry-funded research and conflict of interest: an analysis of research sponsored by the tobacco industry through the Center for Indoor Air Research. J Health Polit Policy Law 1996; 21: 515-42.

3 . http://www.prevention.ch/rylanderpm.htm [accessed 14 May 2003]

4 . Koo LC, Kabat GC, Rylander R, Tominaga S, Kato I, Ho JH. Dietary and lifestyle correlates of passive smoking in Hong Kong, Japan, Sweden, and the U.S.A. Soc Sci Med 1997; 45: 159-69.

5 . http://www.prevention.ch/ryjueng130103.htm [accessed 14 May 2003]

Competing interests: As editor of the European Journal of Public Health, MM published another paper by authors funded by the Center for Indoor Air Research. This publication was the centre of a long-running dispute between the journal and the authors

concerning undeclared conflicts of interest. It led to his involvement as a witness in a lengthy legal dispute that has recently been resolved (referred to in response). He has received funding from several national and international agencies for work on tobacco control.

Flawed study from the outset 16 May 2003

Jayant S Vaidya,
Hon Lecturer and Specialits Registrar
University College London, Dept of Surgery, W1W 7EJ

Send response to journal:
Re: Flawed study from the outset

Email Jayant S Vaidya:
mailto:j.vaidya@ucl.ac.uk?subject=Re:+Flawed+study+from+the+outset

Dear Sir/Madame

There is a major flaw in the study and the Editors may wish to consider a public retraction.

This study assumes that there is a considerable difference in the exposure to ETS of never smokers' spouse compared to ever smoker's spouse. This is obviously not true.

Most never smoker's spouses would have been exposed to considerable ETS before the late 1990s, when the general exposure to ETS in California started reducing. It would be only in the last 3-4 years of the 39-year study when the ETS exposure to workplace might have been so reduced that there might be a difference in the two groups.

So for most of the data, assuming the spouses meet in their non- working hours, they would be exposed to each other- for typically 2-4 hours a day (assuming a 11 hour work+travel and a 9 hour sleep+eat+bath etc.), whereas they would be exposed to ETS at work for up to 8-10 hours.

Thus the study is comparing a 8-10 hour exposure to ETS among spouses of 'never' smokers to a 12 hour exposure to tobacco smoke among spouses of 'ever' smokers. Assuming a 30% increased mortality for passive smoking and assuming never smokers are exposed to ETS for about 10 hours when they are not with their spouse, compared to 12 hours by spouses of ever smokers, the difference in mortality between the groups should be about ((12- 10)/12) x 30= 5%. In addition, there would be many quitters among the ever smokers - thus reducing the ETS to the spouse and many occasional smokers (mainly at the time when they met their spouse) among the never smokers - increasing the ETS exposure to spouse. Despite the large size of the study, it is well known that a 5% difference in RR is extremely difficult to demonstrate in epidemiological studies, and especially in this study, inability to find a difference especially when only a tiny difference was expected cannot be taken as absence of a difference.

There is no doubt that however flawed this study, unless it is retracted by the BMJ, tobacco industry will use it extensively to promote their vigorous opposition to anti-smoking legislation in general, and anti-ETS laws in specific. Of course they have an urgent need to replace their loss of customer base of about 10,000 to 20,000 per day with new recruits of young smokers.

Competing interests: None declared

Risks for passive smoking are likely to be underestimated.

16 May 2003

Trevor LP Watts,
Senior Lecturer and Consultant in Periodontology
Guy's King's and St Thomas' Dental Institute, London, UK, SE1 9RT

Send response to journal:
Re: Risks for passive smoking are likely to be underestimated.

Email Trevor LP Watts:
mailto:trevor.watts@kcl.ac.uk?subject=Re:+Risks+for+passive+smoking+are+likely+to+be+underestimated.

I think this study (1) suffers from the same bias towards reducing the estimated effect of passive smoking that all previous papers have had.

I sent the following rapid response to a previous paper of Copas and Shi(2) on possible overestimation of risks associated with passive smoking.

I have long been concerned that all passive smoking studies actually underestimate true risks for the following reasons:

1. Few non-smokers are not frequently exposed to tobacco smoke in their daily activities. I live in a non-smoking house, travel on non- smoking trains, and work in non-smoking buildings. Yet I am exposed to tobacco smoke perhaps 15-20 minutes of each day, for instance when waiting for trains to arrive, entering and leaving my workplace, walking near smokers in towns and when going shopping. It may be impossible to find true negative control subjects for passive smoking studies.

2. The position in my childhood was far worse, with frequent exposure on public transport and many other places; this will have affected many control subjects in past studies.

3. Some subjected to passive smoking will undoubtedly become nicotine addicts, perhaps as children, and therefore become smokers themselves. I know of no good estimate of this risk and the subsequent damage.

4. As well as the serious risk of addiction, it is also unlikely for the above reasons that any study using realistic controls has been able to estimate the absolute effect of exposure versus total non-exposure to tobacco smoke.

The effects of passive smoking therefore may be more serious than any studies have shown so far, publication bias notwithstanding. It is also likely that effects of smoking on smokers have been underestimated.

I have no competing interests.

1. Enstrom JE, Kabat GC. Environmental tobacco smoke and tobacco related mortality in a prospective study of Californians, 1960-98. BMJ 2003;326:1057.

2. Copas JB, Shi JQ. Reanalysis of epidemiological evidence on lung cancer and passive smoking. BMJ 2000; 320: 417-418.

Competing interests: None declared

Re: Second-hand smokescreens 16 May 2003

Brian David Porter,
Manager of an NHS Smoking Cessation Service
West Lincolnshire PCT,
Healthy Communities, LN5 7JH

Send response to journal:
Re: Re: Second-hand smokescreens

Email Brian David Porter, et al.:
mailto:brian.porter@lincolnshire.gov.uk?subject=Re:+Re:+Second-hand+smokescreens

Medical focussed research only tells part of the story. Commercial links with research inevitably have influence, and that includes tobacco and pharmaceutical companies. However, society gets the research projects that these companies are funding, not necessarily the ones that are really needed.

Medical health issues simply add further weight to the drive towards smokefree places. The argument about the LEVELof health risk is facile. There IS an health risk, but there are other important issues.

Most smokers know the health risks, and many accept that second-hand smoke has health risks on, for example, their children. But they still smoke - i.e. cognitive dissonance and because they are addicted.

Some of the most important issues around second-hand smoke have nothing to do with medical health. Stinging eyes, tobacco ash, unpleasant stale smells, dirty hair and clothing are examples of things that smokefree people don't like, or want. Second-hand smoke imposes itself on people without discrimination, removing from non-smokers their choice of access to clean air in many public places. The majority non-smoking population wants smokefree public places. What's the problem?

Competing interests: None declared

Irresponsible journalism 16 May 2003

Dominic C Horne,
GP Principal
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Send response to journal:
Re: Irresponsible journalism

Email Dominic C Horne:
mailto:dominic.horne@huntly.grampian.scot.nhs.uk?subject=Re:+Irresponsible+journalism

I was genuinely shocked to see this splashed across the front page of this week's BMJ, tabloid-style. An industry-sponsored, methodologically flawed study with inconclusive results but with major potential public health implications especially once the lay press get hold of it. 'Passive smoking may not kill': how much would the tobacco industry pay for such a soundbite in a major peer-reviewed medical journal? Since when did I pay my subscription so that you could do their dirty work for them?

Competing interests: None declared

Agreeing the limits of conflict of interest 17 May 2003

richard horton,
editor
the lancet

Send response to journal:
Re: Agreeing the limits of conflict of interest

Email richard horton:
mailto:richard.horton@lancet.com?subject=Re:+Agreeing+the+limits+of+conflict+of+interest

The paper by Enstrom and Kabat, sponsored as it is from tobacco- industry related funds, raises an issue that affects all papers with conflicts of interest, one that we at The Lancet struggle with in almost every issue - namely, how much conflict of interest can editors reasonably allow before the findings and interpretation of a particular study are rendered unsafe or, at the very least, too uncertain to be a substantive scientific contribution? In our experience this issue is especially relevant for pharmaceutical industry sponsored studies.

Most readers of medical journals might agree that if every author of a paper held US$1million worth of stock in a product that was the subject of their report, and that if the paper described a beneficial effect of the product, an effect that was likely to substantially increase the personal wealth of each author, then an unbiased interpretation of the study would be almost impossible, no matter what the claims for its validity. As an editor, there would be very little point publishing such a study, since it would be immediately disregarded by most readers. Certainly, we have rejected papers - research and review - because we have judged the personal entanglements of authors just too great to sustain the independence of their work, and so its integrity, from the sponsor.

My first question, then, is whether our policy is fair. Martin Jarvis is reported in The Independent today as saying that "one must not take the view that anything which has got any association with the industry is wrong". If, as a community, we share this view, then The Lancet's policy is clearly unfair since we judge that some associations with industry are simply too deep to deliver a believable interpretation.

But if we feel that there really is a limit to the degree of conflict that we judge reasonable, as some responses to the Enstrom and Kabat paper seem to suggest - eg, Amanda Sandford of Ash: "Questions will inevitably be asked about the decision to publish research conducted by scientists in the pay of the tobacco industry" (The Independent) - then criticism should not be directed at the authors, and still less at the editors of the BMJ, but instead to the entire medical community for having such imprecise thinking over conflicts of interest. In pharma-sponsored studies, we mostly allow conflicts provided they are reported accurately. We deplore them in tobacco-sponsored research. One might argue that these sources of funding are qualitatively different - the first does not set out to sell a product knowing that it kills, while the second surely does. But there are many examples of how both tobacco and pharma have tried to undermine the independence and rigour of research, deliberately bias policy makers, gouge grotesquely huge profit from disease, and so on.

The solution that some editors have implemented for pharma-sponsored studies is to require a statement about the role of the funding source in the design, conduct, analysis, and reporting of the data. We publish such a statement for all primary research, irrespective of who the sponsor might be (for-profit, not-for-profit etc). No such statement appears in the Enstrom and Kabat paper - would this have helped readers judge the safety and reliability of their research?

Finally, could this paper therefore provide a useful opportunity for us all to clarify what is an acceptable conflict - for readers, researchers, and editors alike - and how that conflict should be

reported? Could we agree also about how to handle these matters during pre- publication peer review (should the extent of the conflict be a factor, in addition to the science, in deciding acceptance or rejection?) - ie, well before they might confuse an already difficult scientific issue of great public concern?

Richard Horton

Competing interests: None declared

Editorial responsibility to publish sound science 17 May 2003

Trish A Fraser,
Adviser to Action on Smoking and Health
102 Clifton Street, London

Send response to journal:
Re: Editorial responsibility to publish sound science

Email Trish A Fraser:
mailto:tfraser@ash.org.uk?subject=Re:+Editorial+responsibility+to+publish+sound+science

Action on Smoking and Health is one of the key health groups promoting smokefree environments and particularly smokefree workplaces in the UK. The Government has shown little or no willingness to protect the health of the public by eliminating tobacco smoke from workplaces and public places. For health advocates working in tobacco control 'clearing the air' in the UK is an extremely difficult task.

The latest study on environmental tobacco smoke by Enstrom and Kabat was therefore viewed with alarm and dismay. This study has not been accepted as having any credibility by any public health experts as there is already an overwhelming body of scientific evidence that has proven the health impact of exposure to secondhand smoke [1] [2] [3] [4] [5]. There is also no question that both authors have been funded in the past, and for this particular study by the tobacco industry, so why did the editors of BMJ deem it their role to publish this article?

If publication of the article was not bad enough, the problem was intensified by the statement 'Passive Smoking may not Kill' on the cover of the journal, followed by an editorial title which included the word 'controversy' when there is no controversy.

The BMJ has now offered the tobacco industry credibility to continue to promote doubt and uncertainty about the health effects of secondhand smoke. This will assist them in their efforts to maintain the status quo of smoky work and public environments as the accepted 'norm'. Is there not a case for the BMJ editors taking some responsibility to provide science that is unbiased and trustworthy?

1. Respiratory health effects of passive smoking: Lung Cancer and other disorders. The report of the US Environmental Protection Agency, 1993.

2.Report of the Scientific Committee on Tobacco and Health. 1998.

3.International Consultation on Environmental Tobacco Smoke (ETS) and Child Health. WHO Tobacco Free Initiative, 1999

4. Health effects of exposure to environmental tobacco smoke. The report of the California Environmental Protection Agency. Smoking and Tobacco Control Monograph 10, National Cancer

Institute, 1999

5. Involuntary Smoking. IARC, 2002.

Competing interests: None declared

Evidence Based Medicine? 17 May 2003

Stephen Novick,
SCMO Old Age Psychiatry
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Send response to journal:
Re: Evidence Based Medicine?

Email Stephen Novick:
mailto:smn@clara.co.uk?subject=Re:+Evidence+Based+Medicine?

Evidence Based Medicine is a wonderful thing. It seems as though one takes the evidence one likes and uses it, and ignores the rest. Evidence which fits with expected theories and ideas is fine, otherwise it is "flawed" or "biased". I'm afraid evidence is evidence and to dismiss it out of hand just because it is not liked does the medical profession a great disservice. If the evidence doesn't seem to fit, then repeat it, don't just dismiss it.

As Karl Popper said, the hypothesis that all swans are white is not strengthened by finding the one thousandth white swan, but is destroyed by finding the first black one.

Competing interests: None declared

The letter BMJ failed to write 17 May 2003

Pascal A. Diethelm,
Director, OxyGenève
CH-1204 Geneva Switzerland

Send response to journal:
Re: The letter BMJ failed to write

Email Pascal A. Diethelm:
mailto:diethelm@oxygeneve.ch?subject=Re:+The+letter+BMJ+failed+to+write

Dear Editor,

If you go to the Philip Morris document web site (www.pmdocs.com), you will find, under Bates No. 2065122062, the letter [1] that BMJ failed to write to James Enstrom and his co-author. It says: "The editors believe that this opinion piece is full of speculative assumptions of doubtful scientific value. We could not judge the merit of your criticisms because your own data and methods were so inadequately described." The letter was written in 1996 by the Deputy Editor of the Journal of the American Medical Association in response to an earlier submission by Enstrom of his tobacco-industry sponsored study.

It is saddening that a prestigious publication such as BMJ has lowered its publication standards to the point of letting a piece of rubbish occupy its columns and amplifying it with a complaisant editorial. It is unqualifiable that such an article should manage to get published just a couple of

days before the opening of the World Health Assembly at which the the Framework Convention for Tobacco Control is scheduled for adoption, at a moment when the tobacco industry deploys its most intensive efforts to undermine the WHO treaty. A treaty, which, coincidentally, says in its Article 8 : "Parties recognize that scientific evidence has unequivocally established that exposure to tobacco smoke causes death, disease and disability."

If BMJ had decided to side with the tobacco industry in what it still considers a "controversy", it couldn't have chosen a better move and a better time.

1. http://www.pmdocs.com/getimg.asp?pgno=0&start=0&docid=2065122062

Competing interests: None declared

ETS - Interpretation of the wider evidence 17 May 2003

Julia A Critchley,
Lecturer in Epidemiology / Research nSynthesis
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Send response to journal:
Re: ETS - Interpretation of the wider evidence

Email Julia A Critchley:
mailto:juliac@liverpool.ac.uk?subject=Re:+ETS+-+Interpretation+of+the+wider+evidence

There appear to me to be several omissions in Enstrom and Kabat's analysis of environmental tobacco smoke (ETS) and mortality (1). First, although they accept that most epidemiological studies have found that ETS has a positive but not statistically significant relation to coronary heart disease and lung cancer, then argue against the use of meta-analysis to establish a causal relation. This is precisely where systematic reviews, and sometimes meta-analysis, can be of considerable benefit. Many studies have found positive relationships between ETS and mortality – combining them provides greater power to establish a statistically significant effect. The authors suggest that publication bias may explain the positive results in other reviews; but unlike small clinical trials and reports, larger cohort studies are more likely to be published regardless of their results (2). They further ignore the heterogeneity between their study results and many others, simply arguing that none of the other cohort studies on ETS have as many strengths, and none has presented as many detailed results. Although this is a large study, that by itself is not an indicator of 'quality'. Larger prospective cohort studies may have greater losses to follow-up, or more misclassification, over time (3). A more useful analysis would put this study in context and attempt to explain why it differed from other published cohort studies.

Second, though the authors discuss misclassification, it still seems likely that this may explain the lack of statistically significant association. The relative risks reported for active smoking and coronary heart disease (Table 10) are lower than those reported from other cohort studies, such as the British Doctor's (4). This may be sufficient to obscure a modest but important increase in risk.

Thirdly, the author's state that the increased risk of coronary heart disease due to active smoking is only 70% (a relative risk of 1.70). Other studies have found risks associated with cigarette smoking considerably higher than this (5). The authors also seem to assume a linear relationship between cigarette smoking and mortality; this is not likely to be the case. Presumably they extrapolated the very low estimates of RR, assuming that ETS is equivalent to smoking one cigarette per day, on this basis. This analysis is not clearly described.

Placing this study in context, it does not overturn established relationships between ETS and

mortality. I would strongly agree with the editorial that the authors 'over-emphasise' the negative nature of their findings.

1)Enstrom JE, Kabat, GC. Environmental tobacco smoke and tobacco related mortality in a prospective study of Californians, 1960-98. BMJ 2003; 326: 1057.

2)Sutton AJ, Duval SJ, Tweedie RL et al. Empirical assessment of effect of publication bias on meta-analysis. BMJ 2000; 320:1574-7.

3)Critchley JA, Unal, B. The Health Effects Associated with Smokeless Tobacco Use: A Systematic Review. Thorax 2003; 58:435-443.

4)Doll R, Peto R, Wheatley K, Gray R, Sutherland I. Mortality in relation to smoking: 40 years' observations on male British doctors. BMJ 1994;309:901-11

5)Jousilahti P, Vartiainen E, Korhonen HJ, Puska P, Tuomilehto J. Is the effect of smoking on the risk for coronary heart disease even stronger than was previously thought? J Cardiovasc Risk 1999;6:293-8.

Competing interests: None declared

Seondhand Smoke Study is Seriously Flawed 17 May 2003

Marty Eckrem,
Program Manager, Coconino County Dept. of Health Services
Flagstaff, AZ 86004

Send response to journal:
Re: Seondhand Smoke Study is Seriously Flawed

Email Marty Eckrem:
mailto:meckrem@flaglink.net?subject=Re:+Seondhand+Smoke+Study+is+Seriously+Flawed

I can not believe that a reputable journal such as the British Medical Journal can seriously print such a flawed study except to increase readership and create controversy. Since very few people were not exposed to secondhand smoke in the 1950s, this study does not have a reliable control group. Also, during the 38 year interval, a vast number of possiblities exist for the participants, so it would be difficult to reliably classify their secondhand smoke exposure. There are over 50 reliable published studies that confirm the increased risk of lung cancer and heart disease for non-smokers married to smokers.

Competing interests: None declared

Lies, Damned Lies and Statistics 17 May 2003

Richard EK Russell,
Consultant Chest Physician
Wexham Park Hospital

Send response to journal:
Re: Lies, Damned Lies and Statistics

Email Richard EK Russell:
mailto:r.russell@imperial.ac.uk?subject=Re:+Lies,+Damned+Lies+and+Statistics

Sir,

Other commentators are better placed to comment on the methodological flaws and the conflicts of interest stated in the study by Enstrom and Kabat. I would like as a "jobbing" chest doctor to make 3 comments. Firstly, we deal with single patients, such that each patient is an "n" of one. Thus although relative risks may be valid and useful for population studies they are not applicable to the patient in front of us at that time. It is not very reassuring for a passive smoker with lung cancer that they are a statistical abnormality!

Secondly, in law any increase in relative risk is considered a causal association. Because passive smoking might cause pulmonary or cardiovascular disease is interpreted,for legal purposes in court, that it did

Finally, this paper will be seen by the Tobacco industry as a great victory, particularly as it was widely reported by the national media in very clear tabloid terms. Unfortunately the accompanying editorial, although an depth and accurate description of the available evidence, was not easily accessible and did not give and clear message which might counter the irresponsible, dogmatic conclusions of the paper.

Competing interests: Member of the British Thoracic Society and active supporter of the British Lung Foundation

Timing of publication 17 May 2003

Jephat Chifamba,
Medical Physiology Lecturer
University of Zimbabwe Medical School Physiology Dept. MP167 Harare Zimbabwe

Send response to journal:
Re: Timing of publication

Email Jephat Chifamba:
mailto:chifamba@medic.uz.ac.zw?subject=Re:+Timing+of+publication

I am a little concerned with the timing of this publication. The Framework Convention on Tobacco Control (FCTC)is being tabled and we have this? Who is fooling who?

Competing interests: None declared

Irresponsible public health message 17 May 2003

Sabina Fatima Hussain,
SpR Public Health
MSc Public Health, London School of hYgiene and Tropical Medicine

Send response to journal:
Re: Irresponsible public health message

Email Sabina Fatima Hussain:
mailto:sabinakhhtar@aol.com?subject=Re:+Irresponsible+public+health+message

Even if the results of this study with all its limitations are given credence, the message that emerges for the non smoker spouse is that, "Do not fret if you share an intimate environment laden with toxic by products and carcinogens with your spouse who smokes. While his risks of

dying of lung cancer, coronary heart disease and chronic obstructive pulmonary disease are sufficiently well established, the wisom derived from this study somehow spares your own chances of dying from the same dreaded killers. You could still suffer the ravages of ill health imposed by various chronic diseases consequent to the fumes inhaled passively but chance of dying from these causes is small!"

The only solace comes from the Declaration of Interest where it is noted with relief that these irresponsible themes have not emerged from research sponsored by Public Funds. The conclusions of the study carry a note of desperate bid by the tobacco industry to survive the market that is being increasingly marginalised by public health activists.

Dr Sabina F Hussain
Specialist Registrar, Public Health

Competing interests: None declared

Give them enough rope 17 May 2003

Daniel F. Hass,
U.S. government employee
Duluth, MN 55807 USA

Send response to journal:
Re: Give them enough rope

Email Daniel F. Hass:
mailto:unklscrufy@chartermi.net?subject=Re:+Give+them+enough+rope

Since none of the American medical journals saw fit to publish this study, congratulations to the BMJ for showing some courage in the face of what's certain to become a firestorm of protest from entrenched anti- tobacco interests.

Although highly entertaining to witness, the righteous indignation displayed here and in the American media from anti-tobacco activists is disingenous at best. Relying on outdated studies, flawed meta-analyses and garden variety junk science in order to further their agenda of social engineering has been a staple of the anti-tobacco industry for decades. Denouncing the results of this study for the same reasons that most anti- tobacco research could--and should--also be denounced is a slap in the face to anyone with even a modest amount of common sense.

As the hue and cry from those with a financial stake in the debate increases, so too does my certainty that Enstrom and Kabat have struck a nerve too long left dormant. The shrill response from anti-tobacco special interest groups can only encourage more attention from the mainstream media, if for no other reason than to discover what all the fuss is about.

The deceptive nature of the "science" behind the anti-tobacco crusade is a story that needs to be told. Perhaps that day has come. To consider that increased scrutiny of previous anti-tobacco research probably wouldn't happen if the anti-tobacco activists weren't making this such an issue is a delicious slice of irony.

Competing interests: None declared

Re: Irresponsible journalism 17 May 2003

Ellen C G Grant,
physician and medical gynaecologist
20 Coombe Ridings, kingston-upon-Thames, Surrey KT2 7JU

Send response to journal:
Re: Re: Irresponsible journalism

Email Ellen C G Grant:
mailto:ellencggrant@onetel.net.uk?subject=Re:+Re:+Irresponsible+journalism

Tobacco smoke kills

Enstrom and Kabat state they did not rule out an effect of environmental tobacco smoke on mortality. They say an increase in coronary artery disease of 30% is generally accepted. This matches their only significant finding of a 30% increase in mortality in women, defined in 1972 and followed 1973-98, whose husbands' smoked 20 cigarettes/day, which was the commonest smoking exposure category in both sexes.

There were 5-6 times more women exposed to spouses' smoking than men but there is no mention of the powerful synergistic effect of using contraceptive and /or menopausal hormones on heart disease and lung cancer.

Most children brought up in the 1930s and 1940s were subject to parental smoking. This also needs to be taken into account. Smoking killed my father at age 72 and, although my mother remained healthy until her sudden death at age 95, my sister was stillborn and I react badly to tobacco smoke. Infertility, recurrent miscarriages, stillbirths, small for -dates babies, sudden infant death syndrome are increased by parental smoking.

I agree that it is irresponsible to minimise the far-reaching effects of tobacco smoke.

Competing interests: None declared

what does this add? 17 May 2003

Paul M Jones,
dietitian
Southern Downs Health Services, Queensland, Australia

Send response to journal:
Re: what does this add?

Email Paul M Jones:
mailto:katpaul@dodo.com.au?subject=Re:+what+does+this+add?

The way the article is written certainly comes across as promoting the tobacco point of view.

Quote from the abstract "Conclusions The results do not support a causal relation between environmental tobacco smoke and tobacco related mortality, although they do not rule out a small effect. The association between exposure to environmental tobacco smoke and coronary heart disease and lung cancer may be considerably weaker than generally believed. "

Many people dont read the whole articles - many may just read the conclusion in the abstract.

"Enviromental tobacco smoke" is a lot more than just whether the spouse smokes. How about studying people who have to work with smoke (eg. hotel workers)? They are probably exposed to

more smoke.

Many people with smoking spouses send them outside to have a smoke, so exposure by this way may be minimal.

This type of research adds nothing to the debate, and plays into the hands of those who want more people addicted to deady poisons - to make a bit more profit for themselves. It should be our role to promote health - not act against it.

All we can do now is hope that this research does not result in any slowing down of smoking being banned in public places. Non-smokers are by far the majority of the population, their interests should be respected.

Competing interests: Competing interests? 1. I have a concern for public health. 2. I think cigarette smoke stinks.

Swimming with sharks 17 May 2003

Paul S McDonald,
Senior Lecturer (Research)
University College Worcester

Send response to journal:
Re: Swimming with sharks

Email Paul S McDonald:
mailto:p.mcdonald@worc.ac.uk?subject=Re:+Swimming+with+sharks

I am surprised that this study has been published in the BMJ. Emstrom & Kabat have been swimming with sharks. Our thoughts should now be with those whose health HAS suffered as a direct consequence of passive smoking. Any future legal action by these people will be be made all the more difficult.

Are the 310 words written under 'funding' and 'competing interests' a BMJ record ?

Competing interests: None declared

Unproven health impact of environmental smoke: A study with low statistical power

17 May 2003

Parthasarathy K S,
Director, Information and Technical Services Division, Atomic Energy Regulatory Board
Mumbai 400094

Send response to journal:
Re: Unproven health impact of environmental smoke: A study with low statistical power

Email Parthasarathy K S:
mailto:ksparth@vsnl.com?
subject=Re:+Unproven+health+impact+of+environmental+smoke:+A++study+with+low+statistical+power

The article by Enstrom and Kabat gave unexpected results.I request a biostatistician to examine the results to see whether the sample size was adequate to give clear results.The study reported by Enstrom and Kabat may not have sufficient statistical power.

A 40 cigarettes -a-day smoker inhales pollutants from over 4kg of tobacco annually.This estimate is based on the assumption that each cigarette contains a gramme of tobacco and about 30% of the smoke from every cigarette enters the lungs.When the lungs of hundreds of people are subjected to such atrocious abuse one or two of them get lung cancer! Obviously lung tissues are made of sterner stuff.

Seventy percent of the smoke from every cigarette remains airborne.These smoke particles have sizes of fraction of a micrometre and will remain for ever in the respirable range.

During passive smoking there is significant dilution. Also the "surface chemistry" of the smoke particle in the passive smoke stream may be very different from that of a fresh, nascent smoke particle directly inhaled by the smoker.

The particle from passive smoke is likely to be less reactive.There are thus several mitigatory mechanisms in place to reduce the carcinogenic potential of side stream smoke.

This would mean that we must get more number of passive smokers in the study to establish the harmful potential of side stream smoke.It is not surprising that relatively fewer passive smokers may be stricken by smoking related diseases.

Competing interests: None declared

Biggest impact on developing countries 17 May 2003

Judith M Mackay,
Director, Asian Consultancy on Tobacco Control
Hong Kong, China (no pc or zipcode)

Send response to journal:
Re: Biggest impact on developing countries

Email Judith M Mackay:
mailto:jmackay@pacific.net.hk?subject=Re:+Biggest+impact+on+developing+countries

The biggest effect of this article, the accompanying editorial, and the front page of the journal, lies not within Europe and North America, but in developing countries in Asia and Africa. Here colleagues, the media, politicians, other decision makers, and the population in general may not have access to the lively debate that the article has stimulated in developed countries, particularly in relation to methadology and tobacco industry funded research.

Developing countries are struggling to introduce tobacco control measures, often in the face of considerable opposition. It is unfortunate that the "take-home" message from this article may be that such measures are probably unnecessary.

The tobacco industry must be delighted with the timing; the very week that delegates from member states are heading to Geneva for the adoption of the Framework Convention on Tobacco Control at the World Health Assembly.

Competing interests: None declared

some thoughts 17 May 2003

martin heilweil, PhD,

retired
retired

Send response to journal:
Re: some thoughts

Email martin heilweil, PhD:
mailto:martin001@juno.com?subject=Re:+some+thoughts

I have a PhD in Social Research from the University of Michigan, and worked for ten years as a biostatistician/ data analyst/ data manager at Memorial Sloan Kettering Cancer Center in New York City. Our work used the same proportional hazards general linear models Cox regressions that this study seems to use. I did not look much at smoking issues but rather other cancer inquiries, although one study of cervical esophageal cancer had 100% (small Ns) of decedents having been smokers. Ouch. I am a believer.

I then worked similarly for a few years in FDA submissions in one or another US pharmaceutical company.

I have long been skeptical of ETS as a cause of mortality. I first became a skeptic when I saw that an early report, early 1990s, probably a meta analysis, moved the goalposts, from a p level of .05 to a p level of .15. This was buried in the background of the report. Nothing since then has been persuasive. Junk science.

I continued as a skeptic when I started to see that who was against ETS: of course the financial interests, via litigation; and who in favor of ETS, financial interests just as great, via tobacco-interest funding.

In the FDA world, all analysis is done double blinded, and occasionally even triple blinded (the researchers don't know which is brand x, and this analysis may even be farmed out, separate from the data collection efforts). This is the gold standard, or the platinum iridium standard for research, and from my FDA days, no study without double blind has merit. That said, we must work with what analysis we have.

But this double blind criterion would silence many of the conflict of interest complaints. It is indeed possible, I submit, to disguise the data and the names of the variables, perhaps toothpaste preference in toothpaste using spouses, and subcontract the work to a third party. When I did physician-based outcome research in cancer intervention (which doctors' patients had better survival?, I analyzed based on numbers, not names, for example.) Note, there are differences between doctors, which we then analyzed in terms of patient characteristics and surgical approaches.

I wish the authors had devoted more time to critiques of the meta analysis data, the inappropriate combination of samples, and the differing assumptions in combined studies. Alas, but this was not their report; their report was their own data.

There are also important points to be made on percentage of followup. The authors come in at 50%, over a lifetime cohort study. While at Sloan Kettering, part of my job was followup on our cancer inpatients, to monitor health, morbidity and mortality. Followup is a complicated process, and cancer facilities were, in the 1980s, rated by percentage of a mandated annual followup successfully done. We maintained a data file called the Cancer Registry. Mortality was included as a followup end point. Our methodology was essentially active letter writing, and data mining within the then-new data integration capabilities in the emerging PC world; I created such an internal network via 'sneaker-net,' and we achieved 90% followup of our active denominator of former cancer inpatients. Barely and exhaustingly. 50% is pretty good, and the authors seem to have addressed self-selection or preferential access, but this is a larger question. Obviously, a dead

population over forty years will be lost to followup more than a living population, but the authors use the standard national death indices, as did we.

All this said, the data are statistically and intuitively overpoweringly persuasive. It has always been unreasonable, that while smokers' pathology is dose (smoking) related, with inhaled dosages themselves varying by dosage (cigarettes smoked, per day and per year) in their pathogenesis, the second hand inhalation is trivial compared to primary inhalation, and yet there has been no inhaled dosage relationships reported in ETS analysis.

Thus the true measure of 'exposure' is not the hours of co-presence in the home with a smoker, but some measure of inhalation. This should be measurable by analysis of exhaled air samples, or more modestly, by comparing ambient air pathogens, in the home or elsewhere, with lung exhalation from smokers themselves. The falloffs in presence of pathogens would be in the order of hundreds or thousands, and these orders of magnitude or exactly the orders of magnitude the EPA uses to discuss environmental contaminant risks, orders of magnitude.

As for the criticisms that codomiciling with ever-smokers is irrelevant, because of external ETS, this should randomize across the two groups, and obscure differences, such that real differences between codomiciling with smokers, vs nonsmokers, is even sharper.

The one writer who expresses concerns that twenty minutes a day of ETS on mass transit is enough to cause cancer, is not to be taken seriously. One doubts just what disease entity is so pathogenic as to be mortal, with twenty minutes a day of exposure. Perhaps SARS, perhaps the 1920s influenza, perhaps bubonic plague. Cancer death is a cumulative not immediate process. Were this 20-minutes and dead relationship to be so, the world of smokers' neighbors would long have been depopulated. This is clearly not so.

Anyway, the questions now move on to peer review, and data reanalysis, and we may hope that science will accept the internal discipline of inconvenient discrepant data and revised conclusions. BJM is to be congratulated for providing this forum.

Competing interests: general skeptic over ETS

Study Objective Flawed--Fatally 17 May 2003

Stephen J. Jay,
Professor of Medicine and Public Health
Indiana University School of Medicine, 1050 Wishard Blvd. RG 4175, Indianapolis, IN. 46202

Send response to journal:
Re: Study Objective Flawed--Fatally

Email Stephen J. Jay:
mailto:sjay@iupui.edu?subject=Re:+Study+Objective+Flawed--Fatally

The fatal flaws in the paper by Enstrom and Kabat (BMJ 2003;326:1057- 0) are evident in the title of the article and in the Objective statement.

The title claims the study is "a prospective study." It is not by any contemporary definition of the term. The failure to apply the same measures and definitions of exposure to ETS over the duration of the study is just one of several egregious deviatons from a credible prospective research design.

The study Objective: "measure the relation between ETS as determied by smoking in spouses and long-term mortality from tobacco related disease," is bizarre and misleading. How can one possibly

expect to determine the relationship between ETS and tobacco-related mortality by defining ETS as "spousal smoking" alone. The authors apparently elected to discount the obvious fact that during the first two decades of the study period the public was awash in ETS, at home, in the workplace and in social gatherings. This problem and the failure to use consistent measures of ETS exposure throughout the study make any interpretation of study findings an excercise in futility.

Competing interests: None declared

Re: Need for clarification on competing interest 17 May 2003

Geoffrey C Kabat
New Rochelle, NY USA

Send response to journal:
Re: Re: Need for clarification on competing interest

Email Geoffrey C Kabat:
mailto:gck1@earthlink.net?subject=Re:+Re:+Need+for+clarification+on+competing+interest

To the Editor: Dr. Enstrom and I will be responding in due course to the letters concerning our paper. For the moment I would like to answer one specific request for clarification in the letter from Dr. McKee.

My role in the paper by Koo et al. [1] was limited to contributing an analysis of data from a large USA-based case-control study of tobacco-related diseases. The idea of the paper was to examine the extent to which exposure to environmental tobacco smoke was associated with dietary and other lifestyle practices in four countries. I had no knowledge of any of the authors' relationships with the tobacco industry, and I received no payment for my work on the paper. As far as I was concerned, this was a legitimate and interesting comparative study of possible confounding of the weak association of passive smoking with lung cancer, which most epidemiologists would acknowledge is a topic deserving of study. The results reported in our paper are consistent with other published reports on the correlates of active smoking [2, 3].

What this one small example shows is that, in the highly charged atmosphere surrounding research on the health effects of tobacco, it is easy to impute dishonest motives to a researcher when these simply were not there.

Scientists, and particularly epidemiologists, who deal with the criteria for judging causality, should be wary of imputing motives based on the flawed logic of guilt by association.

Geoffrey Kabat, Ph.D., M.Sc.
New Rochelle, NY, USA

1. Koo LC, Kabat GC, Rylander R, Tominaga S, Kato I, Ho JHC. Dietary and lifestyle correlates of passive smoking in Hong Kong, Japan, Sweden, and the U.S.A. Soc Sci Med 1997; 45: 159-169.

2. Hebert JR, Kabat GC. Differences in dietary intake associated with smoking status. Europ J Clin Nutrit 1990; 44: 185-193.

3. Subar AF, Harlan LC, Mattson ME. Food and nutrient intake differences between smokers and non-smokers in the U.S. Am J Public Health 1990; 80: 1323-1329.

Competing interests: As stated at the end of the article by Enstrom and Kabat.

Nothing new from the antismoking front 17 May 2003

Wiel M Maessen,
Board member of Forces International
Nethelands

Send response to journal:
Re: Nothing new from the antismoking front

Email Wiel M Maessen:
mailto:wiel@forces-nl.org?subject=Re:+Nothing+new+from+the+antismoking+front

We have seen for years that anti-tobacco research has been funded by Big Pharma and all anti-smoker organisations accpet it. More than a quarter of a billion dollars has been invested in anti-tobacco research through the Robert Wood Johnson Foundation, a 'charitative' daughter company of Johnson&Johnson, the ones who sold Nicotine Replacement Therapy products, so being the competitor of the tobacco industry on the nicotine market.

In later years, bags of money from the Master Settlement Agreement were used to conduct research on anti-tobacco. Most of these studies were conducted under the responsibility of anti-smoking organisations themselves. All these studies were accepted by the anti-tobacco industry as being real. Nobody complained about competing interests. And now we see anti-tobbaco complain about a study where only a minimal part was funded by Big Tobacco but most of it by the American Cancer Society and from the Prop 99 surtax funds from the Tobacco-Related Disease Research Program?

To us it looks like studies are only accepted by the stakeholders in the Health Industry when it fits their political objectives. Maybe it's about time that the medical establishment goes back to it's roots of helping people when they need it and not being engaged in social reengineering based on junk science....

Competing interests: None declared

Re: Agreeing the limits of conflict of interest 17 May 2003

Clive D Bates,
Personal capacity
London N16 5UF

Send response to journal:
Re: Re: Agreeing the limits of conflict of interest

Email Clive D Bates:
mailto:clive.bates@dial.pipex.com?subject=Re:+Re:+Agreeing+the+limits+of+conflict+of+interest

There are two additional dimensions to the debate about conflicts of interest: quality of peer review and media spin.

Where there is a conflict of interest and, as in this case, very obvious serious public policy implications, a journal has more than the usual duty to have a rigorous peer review. This is an important safe-guard and it is this tough gatekeeping role that can allow some conflicts of interest to be tolerated if the work is good enough and stands on its own merits. I've never liked the idea of excluding work on the basis of its origin, but conflicts of interest (especially those involving the tobacco industry) should have everyone involved on red alert. Several commentators, including the American Cancer Society which owns the data, have pointed out basic flaws in the study, such as

the contamination of the control group. One rapid response claims out that JAMA rejected an earlier version of this study. But it was still accepted. So how good was the peer review for this paper?

Publication in the BMJ is 'capture' of one of the major fortresses of evidence-based practice by the tobacco industry and its lobbyists - and it will serve them very well all around the world for many years. How careful was the BMJ and its reviewers when they handed them this trophy? John Maynard Keynes is famous for saying "When the facts change, I change my mind. What do you do?". If the understanding of the facts on second hand smoke had indeed changed then the tobacco control community needs to be prepared to rethink. But it doesn't look like the facts have changed at all, just that the BMJ has let a shoddy study assume the status of 'facts'.

The second point is about what the BMJ did to frame the story for the media. The BMJ's press office sends out a press release each Tuesday or Wednesday to thousands of journalists around the world giving a line on the main headline-making studies in the issue that comes out at the weekend for reporting in Friday's media. The objective of a press release is to gain as much media attention as possible for the journal's stories. That objective is easily in conflict with sober scientific reporting of data and findings, but too easily aligned with the objectives of the funders of controversial research. Yes, the tobacco industry and the BMJ are together on this - they both want lots of lovely press coverage.

The press release is not subject to peer review and necessarily relies on shorthand to communicate the scientific data to journalists - words like 'significant' can easily lose their technical meaning in transmission from the journal article to the press release and onto the newspaper page. Yet it is the press release that will have determined how the story is reported and how the headlines are written. It is the source of authority because it is the BMJ saying it and no health correspondednt has to argue with his or her editor about that. So what was in the press release?

I would like to invite the BMJ to post on the web site: 1. The press release 2. The peer reviewers' comments (anonymously)

My hunch is that the BMJ is being challenged for the wrong things... conflicts of interest are important, but they are less important when there is rigorous peer review and careful reporting of the findings.

It pains me to say all this, but it comes only two weeks after the BMJ published what must rank as the most facile and unscientific editorial ever written on the subject of cannabis (2nd May).

Exasperating!

Clive Bates

Not a competing interest, but I would like to disclose that I was director of Action on Smoking and Health (UK) until March 2003, and now writing strictly in a personal capacity.

Competing interests: None declared

Reply to Bates 17 May 2003

Tony Delamothe,
web editor, bmj.com
BMJ

Send response to journal:
Re: Reply to Bates

Email Tony Delamothe:
mailto:tdelamothe@bmj.com?subject=Re:+Reply+to+Bates

The press release was published at the time of the paper:
http://bmj.com/content/vol326/issue7398/press_release.shtml#1

(Make sure you include all the way up to #1)

We intend publishing details of the paper's peer review next week.

Competing interests: I am employed by the BMJ, which published the study.

From hero to pariah in one easy jump 18 May 2003

Richard Smith,
Editor
BMJ

Send response to journal:
Re: From hero to pariah in one easy jump

Email Richard Smith:
mailto:rsmith@bmj.com?subject=Re:+From+hero+to+pariah+in+one+easy+jump

Not long ago I was something of a hero of the antitobacco movement-- because I resigned my professorship at Nottingham University when it accepted money from British American Tobacco. I felt somewhat embarrassed by the whole episode. I was no hero. But now I'm a pariah for publishing a piece of research funded by the tobacco industry. Because of some sort of personality defect that is common among editors I'm more attracted to being a pariah than a hero, but I don't think that I deserve to be a pariah.

We long ago decided that we would not have a blanket policy of refusing to publish research funded by the tobacco industry, as some journals have done. (1) Our argument was that a ban would be antiscience, systematically distorting the scientific record.

I would try to dissuade anybody from accepting tobacco company money, and I resigned from Nottingham because it did so. Isn't it thus hypocritical to publish research funded by the industry? To my mind it isn't. With some difficulty, I'm setting the ethic that all science should be published above the ethic that you shouldn't take money from the tobacco industry. Once the research has been done it should be published, and if it passes our peer review process it can be published in the BMJ.

Our way of making decision on research papers is first to ask if we are interested in the question. We are certainly interested in the question of whether passive soming kills, and it's clear to us that the question has not been definitively answered. Indeed, it may well never be answered definitively. It's a hard question,and our methods are inadequate. We then peer review the study. Two top epidemiologists-- including George Davey-Smith--reviewed the paper. Then the paper went to our hanging committee, which always includes a statistician as well as practising doctors and some of us. Everybody reads every word of every paper. We asked for extensive changes to the paper, and the paper we published was different from the paper submitted--which is usually the case.

We are planning to post on our website all the comments of the reviewers, our statistician, and the hanging committee. I hope that they will be up soon after the weekend.

Of course the paper has flaws --all papers do-- but it also has considerable strengths-- long follow up, large sample size, and more complete follow up than many such studies. I find it distrubring that so many people and organisatons --including the BMA, our owners-- refer to the flaws in the study without specifying what they are.

We judged this paper to be a useful contribution to an important debate. We may be wrong, as we are are with many papers. That's science. But I remain convinced that it would have been wrong to reject the study simply because it was funded by the tobacco industry.

Richard Smith Editor, BMJ

(1) Roberts J, Smith R. Publishing research supported by the tobacco industry. BMJ 1996; 312: 133-134.

Competing interests: I'm the editor of the BMJ and acountable for all that it publishes.

Children are not exposed to parental smoke??!! 18 May 2003

Andrew J Fall,
Consultant Paediatrician
James Cook University Hospital, Middlesbrough, TS4 3BW

Send response to journal:
Re: Children are not exposed to parental smoke??!!

Email Andrew J Fall:
mailto:Andrew.Fall@stees.nhs.uk?subject=Re:+Children+are+not+exposed+to+parental+smoke??!!

I share the multiple concerns raised by many others more erudite than I regarding the publication of this paper. My patients' parents however, provide me with a possible explanation for the study's findings in at least some subjects. The standard response to the question "does anyone smoke at home?" is "yes, but never in the house; only outside". I am sure other paediatricians also hear this on a daily basis but I doubt that I am alone in having some doubt as to the veracity of these proffered replies.

Competing interests: None declared

What in the world were you thinking? 18 May 2003

Sera Kirk,
None
Vancouver, British Columbia, Canada V6R 3R1

Send response to journal:
Re: What in the world were you thinking?

Email Sera Kirk:
mailto:serakirk@hotmail.com?subject=Re:+What+in+the+world+were+you+thinking?

I'm not a medical professional; however, I do try to stay informed. What possessed a respected publication such as the BMJ to go tabloid, publishing a sensationalist paper based on research with

serious design flaws? Ask any 5-year-olds on the street whether comparing passive smokers to other passive smokers would produce meaningful results about the effects of secondhand smoke and I'm certain that between 90-100% would understand how potty that sounds. It is pretty obvious that all this study proves is that secondhand smoke exposure in the home is no worse than secondhand smoke exposure anywhere else. Yet somehow this slipped by your distinguished editorial board.

Once the media loses interest in this paper, it will be referred to only by desperate smokers in denial and tobacco executives who, since it was struck down, can no longer refer to Judge Osteen's ignorant decision about the EPA's study on secondhand smoke.

One thing is clear - with the FCTC looming and an ever-increasing demand for smoking bans, the tobacco industry is getting desperate.

Competing interests: None declared

Re: Nothing new from the antismoking front 18 May 2003

David F. Copeland,
MGH
Montreal H3Z2E6

Send response to journal:
Re: Re: Nothing new from the antismoking front

Email David F. Copeland:
mailto:dcopela@attglobal.net?subject=Re:+Re:+Nothing+new+from+the+antismoking+front

How ironic that the BMJ accepts without comment a letter from someone who declares "no competing interests" but is a "Board member of Forces International"?

Does no one at the BMJ know anything about this tobacco-industry advocacy group? Is it possible that you are so naive you do not understand how sophisticated the tobacco industry's advocates are?

At least look them up (e.g. Google) and see for yourselves!

There is no real controversy: promotion of tobacco use is psychopathy, and the illnesses resulting from tobacco use overwhelm most other causes in 'developed' countries.

The misleading and tendentiously-reported study you have published is a strategic triumph for the marketing genii whose victims number not only in the millions of deaths per annum, but whose product provides ample work for physicians and others treating respiratory disease. You can be sure they will make good use of this 'product placement'.

No wonder we physicians have made hardly a dent in tobacco- related illnesses. When dealing with the duplicitousness and implacability of tobacco advocates, we are simply out of our depth.

Competing interests: The health of my patients, relatives, and friends. Hundreds of which have succumbed to a product deceitfully advertised, 'normalised' by 'spin' in media, and in some cases even in otherwise reputable -- but perhaps somewhat naive -- medical journals. Physicians for a Smoke-free Canada;
American College of Allergy Asthma & Immunology; American Academy of Allergy Asthma & Immunology;

Canadian Society of Allergy and Clinical Immunology; Association des Allergologues et Immunologues du Québec;
Ontario Allergy Society.

FINALLY !! Truth Comes Out of the Closet. 18 May 2003

Stephen Hartwell,
SmokersRightsCanada.Org
M3A1Z1

Send response to journal:
Re: FINALLY !! Truth Comes Out of the Closet.

Email Stephen Hartwell:
mailto:admin@smokersrightscanada.org?subject=Re:+FINALLY+!!+Truth+Comes+Out+of+the+Closet.

FINALLY !! Some Truth, Big Truth, about ETS.

No Measurable Causal Risk from ETS.

We smokers have waited a long time to see this Truth in a Major global Medical Journal.

Dr. Simoncini, MD, of Italy says it best. Paraphrasing -

Not 1 Single Illness nor Death has ever yet been assigned as Etiologically Caused by ETS.

And, now we have this 39 year study also proving the same thing.

Thank You British Medical Journal for your Courage to print the Truth.

Stephen Hartwell www.smokersrightscanada.org

Competing interests: None declared

Funding by EPA,, CDC, WHO equally "tainted" 18 May 2003

Natalie P.R. Sirkin,
devoted reader
44 Big Trail, Sherman, CT 06784

Send response to journal:
Re: Funding by EPA,, CDC, WHO equally "tainted"

Email Natalie P.R. Sirkin:
mailto:GNSirkin@aol.com?subject=Re:+Funding+by+EPA,,+CDC,+WHO+equally+"tainted"

Readers dismiss this study because it is partially funded by the tobacco industry. Who else would fund a study likely to produce these results?

EPA showed its bias when for five years prior to publication of its 1992 study, it was declaring in official releases that ETS causes lung cancer. Similarly, the doctor in charge of the CDC study of cotinine admitted to us in a telephone call that CDC had undertaken the study "to help" the anti-ETS and anti-smoking groups.

The question facing us is whether the authors changed their results or methodology because of the source of their funds. No one in the responses you have published has suggested that they did.

We can't expect the EPA to fund a study like this. If the BMJ and other journals decline to publish studies funded by a biased source, they would not publish studies funded by EPA or WHO or CDC.

Critics forget or never knew that EPA's meta-analysis of ETS of 1992, despite its high selectivity and massive statistical errors (described in detail by the Federal District Court which overturned the EPA decision) still showed a relative risk of a tiny 1.19 for lung cancer. WHO's study of ETS in six countries showed nearly identical results. EPA and WHO are in the same position as the tobacco industry, having a keen interest in the outcome of their studies.

Clearly, this study should be judged on its own merits and not on the source of its funds. I thank BMJ for publishing it. I thank the tobacco industry for financing it.

Competing interests: None declared

When conflict of interest becomes unacceptable 18 May 2003

Deborah Arnott,
Director ASH (Action on Smoking and Health)
102 Clifton Street London EC2A 4HW

Send response to journal:
Re: When conflict of interest becomes unacceptable

Email Deborah Arnott:
mailto:deborah.arnott@ash.org.uk?subject=Re:+When+conflict+of+interest+becomes+unacceptable

Deborah Arnott Director, ASH (Action on Smoking and Health)

The tobacco industry has a long and discreditable history that brings into question any research which has received industry funding. This is why ASH believes that the BMJ were wrong in publishing this article without making clear the tobacco industry connection. But worse still this study is not academically credible and it is hard to understand how the article passed the BMJ's peer review process. The data used was that of the American Cancer Society whose criticisms of the study are damning. To quote Michael J. Thun, MD, the Society's national vice president of epidemiology and surveillance research.: "We are appalled that the tobacco industry has succeeded in giving visibility to a study with so many problems it literally failed to get a government grant," ... "The American Cancer Society welcomes thoughtful, independent peer review of our data. But this study is neither reliable nor independent." [1]

Publication of the report has given rise to media headlines such as "Passive Smoking 'Not so deadly'" and "Passive smoking may not damage your Health". And the tobacco industry is already misusing its findings. Tim Lord, chief executive of the Tobacco Manufacturers Association has been publicly quoted as saying, "This is a large and very important study....taking the evidence as a whole the inevitable conclusion is that claims made about the potentially harmful effects of passive smoking have indeed been overstated."

This is just the latest attempt by the industry to undermine the considerable body of evidence that secondhand or environmental tobacco smoke (ETS) is a major health hazard. the 1990s as Philip Morris ran a multi-million dollar campaign to undermine a study on ETS undertaken by the International Agency for Research on Cancer, an affiliate of the WHO. Targeted at researchers, the media and government its aims were to: * "Delay the progress and/or release of the study.

* Affect the wording of its conclusions and official statement of results.
* Neutralize possible negative results of the study, particularly as a regulatory tool.
* Counteract the potential impact of the study on governmental policy, public opinion, and actions by private employers and proprietors." [2]

More recently an assessment of 106 review articles on passive smoking found that 37% concluded that passive smoking was not harmful to health, 74% of which were written by authors with industry links. In multiple logistic regression analyses the only factor associated with concluding that passive smoking was not harmful was affiliations with the tobacco industry. The conclusion was that authors of review articles should disclose potential financial conflicts of interest and that an authors affiliations should be considered when trying to judge an articles' conclusions.[3]

It appears that in this case the BMJ's desire for publicity and controversy may have undermined its professional standards to an unacceptable degree. We call on the editor of the BMJ to publicly retract the article and publish the American Cancer Society's detailed criticisms in the next edition.

Deborah Arnott deborah.arnott@ash.org.uk

[1] The detailed background to these conclusions can be found on the society's website. http://www.cancer.org/docroot/MED/content/MED_2_1x_American_Cancer_Society_Condemns_Tot

[2] Philip Morris 1993 http://bmj.com/cgi/eletters/326/7398/www.pmdocs.com/getallimg.asp?DOCID=2501341817/1823 Bates Number 2501341817 -23 See also "Trust us we're the Tobacco Industry" April 2001 published by the Campaign for Tobacco-Free Kids and ASH UK.

[3] Barnes D. and Bero L. Why Review Articles on the Health Effects of Passive Smoking Reach Different Conclusions JAMA Vol. 279 no. 19, May 20 1998

Competing interests: None declared

Did non-smokers REALLY avoid 18 May 2003

Simon Chapman,
Editor, Tobacco Control
University of Sydney

Send response to journal:
Re: Did non-smokers REALLY avoid

Email Simon Chapman:
mailto:simonchapman@health.usyd.edu.au?subject=Re:+Did+non-smokers+REALLY+avoid

A press release issued by the American Cancer Society prior to the web release of this paper quoted Michael Thun MD, the Society's vice president of epidemiology and surveillance research. His comments bear close inspection together with the full version of the Endstrom/Kabat article as found on the BMJ website. I quote Thun's comments at length:

"the study by Enstrom and Kabat is not reliable or informative for several reasons:

... The critical flaw of this study is its crude assessment of exposure and inability to distinguish people who were exposed to secondhand smoke from those who were not at various points in the follow- up. This is especially problematic in this study, because: a) Participants were enrolled in 1959, when exposure to secondhand smoke was so pervasive that virtually everyone was exposed to ETS, whether or not they were married to a smoker. b) No information was collected on other

sources of ETS exposure besides spousal smoking. c) No information on smoking by the spouse after 1972 was included in the analysis, even though the observation period continued another 26 years, through 1998, so any smokers who quit between 1972 and 1998 would still have been counted as smokers. d) Study participants were on average 52 years old at enrollment. Many spouses who reported smoking in 1959 would have died, quit smoking, or ended the marriage during the 38-year follow-up, yet their surviving partners are still classified as "exposed" to ETS in this analysis."

Points (c) and (d) would appear to be at odds with information contained in the article. Table 5 provides data partitioned for years of spousal smoking exposure (1-19 years; 20-39 years; 40-80 years). Thus, those surviving never-smokers who had at some time lived with at least one smoker (their spouse/partner) and who completed the 1999 follow-up could have accounted for (smoking) spousal death, divorce or quitting in their answers by stating how many years they were thus exposed.

If my reading of this is correct, Thun's criticism of the paper in this respect would appear to be void. However, this information was not carried in the paper version of the paper which was circulated to the press and came into the hands of many people working in tobacco control before the Friday embargo lifted and the full version appeared on-line. This incomplete information would appear to have been the basis for much of the initial criticism of the study's alleged faults.

Further, the paper states that the 1999 cohort were asked "In your work or daily life, are (were) you regularly exposed to cigarette smoke from others?" and that "The 1999 questionnaire showed that the smoking status of spouses was directly related to a history of total exposure to ETS."

The major concerns about the authors' industry affiliations remain. However, if the credibility of the study is to be challenged on the basis of its methods and findings, it would seem that further information should be provided by the authors on the data they used to support their statement about the "direct relation" of domestic ETS exposure with total ETS exposure. Given the ubiquity of smoking in workplaces and indoor leisure venues in the 1960s and most of the 1970s and 1980s, I am struggling to conceive of a hypothesis which would suggest that non- smoking couples would have avoided significant ETS exposure.

Competing interests: I am editor of Tobacco Control, a journal whose name implies a dedicated interest in controlling tobacco use and the diseases caused by it.

Whither epidemiologic reporting? 18 May 2003

Gio B. Gori,
Director
The Health Policy Center, 6704 Barr Road, bethesda, MD 20816, USA

Send response to journal:
Re: Whither epidemiologic reporting?

Email Gio B. Gori:
mailto:gorigb@msn.com?subject=Re:+Whither+epidemiologic+reporting?

Bully for the BMJ Editors, probably the only fearless editorial crew left in the world. Were they suborned by Big Tobacco? Spin scoundrels likely will ventilate as such, but who is Big Tobacco anyway? In 2001 the US tobacco industry made close to $ 9 billion in pretax profit, but provided an assortment of federal and state governments with nearly $ 40 billion in revenue and settlement fees, in effect becoming a nationalized tax collector. No wonder cigarettes heve not been made illegal, despite the unmatched risks. When recently an Illinois judge threatened bankruptcy, who rescued the beleaguered Philip Morris if not a gang of state Attorney Generals? Is government

money clean? Is the antismoking crusade not supported by special interests? And yet BMJ reports on line several hostile comments to the Enstrom & Kabat paper, by people on the public or charity doles who declare no conflict of interest. The nerve!

Why should this paper not have been published, as most critics advocate? How does the paper compare to the some 100 papers on environmental tobacco smoke (ETS) that BMJ and other prestigious journals have published in the last quarter century? Enstrom & Kabat present a rare prospective cohort analysis from a database that the American Cancer Society has found useful and valid for many other studies. Their method of analysis is mainstream and transparent, and relies on disease incidence and individual recall of lifetime exposure to ETS.

Not so for other so called studies of ETS, which virtually all rely on case-control models. Those studies do not compare incidences of disease, but dare to infer causality from slight differences in the recall of lifetime exposure between cases with, and controls without the disease of interest. Recalls are elicited by asking "how many cigarettes were smoked in your presence over the last 20, 40...60 years; how long were you exposed to ETS over those years, etc." Vague recollections are recorded casually as precise digits, and statistically insignificant odds ratios are integrated by selective meta-analyses into overall mean below 1.20. The incongruous and stupefying assumption is made that a supposed 20% excess exposure caused disease in all cases, but none in the 100% exposed controls. An assumption fully incompatible with the further claim of a 20% increased risk, and the preposterous paradox of an excess risk over the imaginary risk of controls who have no disease at all.

Intuitively, a recall elicited after the onset of a disease - lung cancer especially - produces greater exposure estimates from case subjects with the disease, making up for a recall bias impossible to control for. Not so for the prospective cohort study of Enstrom & Kabat, which relies on testable differences of disease incidence and untainted exposure recalls obtained before the onset of disease. If there is an objection, for all its improvements the study is still inadequate to determine the presence or absence of risk, as the authors point out. In truth no epidemiologic study could be mounted to determine unequivocally the presence or absence of an ETS risk. The futility of scouting for possible microbes through a telescope should have been apparent after the first few ETS studies long time ago, thus saving sizable wasted monies and resources. Legislators, regulators, and public health czars may plead the ignorance of those who rely on the hearsay of presumed experts. Yet epidemiologists cannot hide from blame, especially when Doll & Peto – the patron saints of the profession – concede that epidemiology is no science but an exercise in imagination. (1) Freedom from tobacco and ETS is a laudable ambition, but does the end ever justify fraudulent means in the public policy of free societies? Is there no shame left?

Gio Batta Gori, DSc, MPH The Health Policy Center Bethesda, MD, 20816, USA gorigb@msn.com

1. Doll R, Peto R. The causes of cancer. J Nat Cancer Inst 1981;66:1218.

The author was director of the Smoking and Health Program of the US National Cancer Institute, for which he received the US Public Health Service Superior Service Award. In becoming a critic of official allegations, he received occasional support from the tobacco industry. In matters of tobacco and health he has sought in vain other support that might equally come without strings attached.

Competing interests: The author was director of the Smoking and Health Program of the US National Cancer Institute, for which he received the US Public Health Service Superior Service Award. In becoming a critic of official allegations, he received occasional support from the tobacco industry. In matters of tobacco and health he has sought in vain other support that might equally come without strings attached.

Consistency - science for sale? 18 May 2003

John R. Polito,
Founder, WhyQuit.com
709 Black Oak Blvd., Summerville, SC, USA 29483

Send response to journal:
Re: Consistency - science for sale?

Email John R. Polito:
mailto:john@whyquit.com?subject=Re:+Consistency+-+science+for+sale?

This outpouring of concern over the integrity of ETS science is healthy but why no similar concerns when NRT pharmaceuticals openly design studies, finance them, handsomely reward chosen researchers, and even provide placebo devices whose inertness is questionable? Why no concern over outrageous new study definitions of abstinence, cessation, medicine, therapy, and placebo, that redefine and predefine how odds ratio victories are declared, generate billions in profits, and, as we learned in March, produce a 93% midyear relapse rate?

Is our motive health or paternalism, moving smoke or ending it, altering risk factors by compelling the addicted to suck deeper, longer and harder, or saving life? Have stakeholders manipulated and used science to infect and mold cessation policy at every level? Are the web sites of many expressing outrage, today, storefronts for the sale of nicotine products that have been found ineffective by a growing number of recent studies? Financial conflicts?

Have we allowed nicotine and gradual weaning marketing to invade, diminish, damage and destroy the utility and credibility of thousands of short-term community cessation programs that were on average twice as effective, and many vastly superior and worthy of study themselves? Why no concern for honesty and integrity when OTC advertisements hide NRT's 93% relapse rate while falsely implying that it is twice as effective as the smoker's neighborhood abrupt cessation program?

Will more English, Scottish, Irish, Australian, New Zealand, Canadian and U.S. lives be lost in 2003 to secondhand smoke, or to mainstream risks following NRT relapse after "trusting" science with that one brief window of confidence, mustered once every three years? I don't know. What I do know is that science, not financial interests, should be defining terms and writing rules, and that peer-review and oversight is in need of more than a band-aid.

John R. Polito

Competing interests: Abrupt cessation program director

Tobacco Toxicity 18 May 2003

Ken B. Jones,
Secondhand Smoke Survivor
P7A 5X5

Send response to journal:
Re: Tobacco Toxicity

Email Ken B. Jones:
mailto:f350togo@yahoo.com?subject=Re:+Tobacco+Toxicity

Editor; British Medical Journal

Re: tobacco toxin exposure

In the late 1940's a healthy, active, intelligent 3 year old boy contracted a life threatening, extremely virulent strain of pneumonia. From birth he was exposed to heavy concentrations of tobacco based airborne toxins. His severely compromised respiratory function provided little, if any, resistance to the pneumococcus infection.

Severe bronchial asthma followed including several brushes with complete respiratory failure. He averaged 8 stays a year in hospitals plus numerous visits to emergency departments to have less severe asthma attacks suppressed by injections of adrenalin.

His exposure to second-hand smoke continued. His lungs became inflamed and started collapsing (spontaneous pneumo-thorax). Luckily?, unlike less fortunate individuals with the same condition, both his lungs never totally collapsed at the same time. Oxygen deprivation compromised his intellect and physical capabilities.

As an adult he had to keep oxygen in his bedroom for his asthma attacks. The onset of his illness predated Canada's Universal Health Care legislation being passed into law. The mental anguish and financial hardship forced upon his parents caused irreparable damage to their health and well being.

His father succumbed to a third heart attack at age 57 after most of a lifetime of near-chain-smoking. After many years of breathing problems his mother predeceased his father by six months. Sliding into a two week long coma, the upper respiratory viral infection ultimately triggered complete respiratory failure. She didn't smoke.

His parents were both extremely intelligent. If they had been armed with information that foretold the decimating effects that smoking could have on their and their family's lives they probably would have made an informed decision.

We are fortunate today to have the knowledge to protect our children. The merest hint of scientific research financing originating from the tobacco industry should have caused red flags and alarm bells to go off at the prestigious British Medical Journal. The dismal track record of "BIG tobacco's" history of deception, falsification of test results, and bald- faced lies should have given your publication pause for concern.

The narrow scope of the data, such as contained in this report, can be skewed to reflect fairly favourable results for tobacco usage. No matter the (surmised) dearth of pertinent data in this specific area, the purporting to endorse this study reflects an unconscionable disregard for the health and well being of the citizens of our planet.

There is irrevocable evidence that the astronomical health care costs directly attributable to tobacco toxins exposure is one of the main factors placing the Canadian health care system in extreme jeopardy. This scenario is being repeated throughout the entire world.

Ontario legislation is in place empowering municipalities and districts to unilaterally BAN smoking from all public and work places. This is not a decision that should be downloaded to the provinces.

All countries' elected representatives need to pass legislation declaring all locations that admit children; shopping malls, restaurants, bowling alleys, taverns, etc. totally non-smoking.

Tobacco smoke is the only dependency that has an instantaneous profound negative health impact on the non-addicted.

The only possible decision that can be made is to protect the health of our youngest citizens.

Thank you for your time.

Ken B. Jones Thunder Bay Ontario, Canada Email: f350togo@yahoo.com

c.c. Prime Minister Jean Chretien

The Globe and Mail

Competing interests: None declared

BMJ-comic or respectable journal? 18 May 2003

▲ ▼ ⏏ Raj Thakkar,
GP Registrar
-

Send response to journal:
Re: BMJ-comic or respectable journal?

Email Raj Thakkar:
mailto:raj_thakkar@hotmail.com?subject=Re:+BMJ-comic+or+respectable+journal?

The British Medical Journal, an international journal that puts British medicine at the forefront, a world leader in healthcare? Or has it finally become a comic amongst modern medical journals? This week's article on passive smoking begs this question and judging by the number of responses to this article, it seems the BMJ has some thinking to do. Radio stations across the country broadcast the article's findings as to whether passive smoking is indeed harmful after all. Surely the BMJ must have realized the consequences of publishing such an article, perhaps not. Many doctor hours have been dented in persuading patients about the dangers of smoking, using passive smoking as a tool to nudge pre- contemplators along the cycle of change. The article merely drives a wedge between healthcare professionals and their patients. Fine if the article was unbiased and useful but to further rub salt into the BMJ's wounds, one of the study's authors received funding from the tobacco industry. Ironic really that this very "rapid response" webpage asks about competing interests. The journal is at risk of loosing credibility as a responsible institution and is guilty of an abuse of power.

Competing interests: None declared

WHAT THE HELL HAVE THESE PEOPLE BEEN SMOKIN'? 19 May 2003

▲ ▼ ⏏ Errol E. POVAH,
Deckhand
B.C. Ferries

Send response to journal:
Re: WHAT THE HELL HAVE THESE PEOPLE BEEN SMOKIN'?

Email Errol E. POVAH:
mailto:grim.reaper@telus.net?subject=Re:+WHAT+THE+HELL+HAVE+THESE+PEOPLE+BEEN+SMOKIN'?

Of course Second Hand Indoor Tobacco Smoke is harmless. And nicotine is NOT addictive.

AND THE EARTH IS FLAT, TOO!

Seriously, folks, when all else (including common bloody sense) fails, let's do this: Let's pretend, just for a moment, that there's not one iota of truth to all the 'bad news' we've been hearing about second- hand smoke (from literally thousands of credible, reliable, unbiased, neutral, conflict-of-interest-free medical and scientific sources) for the last couple of decades....let's pretend 'we just don't know if it's harmful or not.'

Daniel F. Hass ("Give them enough rope") and FARCES' (Fighting Against Restrictions and Controls on Environmental Smoke) own Wiel M. Maessen ("Nothing new from the anti-smoking front") shouldn't have any problem with this 'pretend' stuff, since they live in Wonderland. But I'm just visiting, very briefly, so I do hope they'll hold off on firing up their celebratory stogies til long after I leave.

Anyway, back to Wonderland for a moment, where WE DON'T KNOW if second-hand smoke is harmful or not.

Whatever happened to that (common-sensical) idea or notion that applies to every other manufacturer/product on the planet, whereby the onus is 100%, completely, totally on the manufacturer to prove, beyond the shadow of a doubt, that the product (or, in this case, the 'by-product') is safe?

Or is that merely yet another pesky irritation that, with a small token of their appreciation ($$$ to 'friendly' politicians, judges, etc.), the tobacco industry is exempt from?

Everyone -- especially the tobacco industry and its army of well-paid puppets -- accepts the fact that, just like mainstream smoke, the industry will NEVER, in a billion years, be able to prove that second-hand smoke is safe. So, in lieu of that, the next best thing is for the industry to continue to turn the 'onus' table, attempting to force the 'health' side to prove that second-hand smoke is harmful (and let's face it, no matter how solid the evidence, we'll never prove it to the industry's satisfaction). And when that doesn't work, the industry relies on its old stand-bys...."cast doubt" and "create delays."

In terms of issues that are critical to the very survival of the tobacco industry, public/workplace smoking (even fighting to weaken/overturn long-established bans) is second only to the industry's virtually unfettered efforts and ability (yes, even in North America, Europe, etc.) to hook and addict generation after generation of underaged kids (their 'new recruits' and/or 'replacement market'), especially in Third World countries.

The time is long past due for EVERYONE to enjoy the same smoke-free atmosphere -- in their home, in their workplace and in EVERY public place -- that most tobacco executives enjoy. It's true: Most tobacco executives don't smoke! And they sure as hell don't want their own to kids to smoke, but everybody else's kids are....well, fair game!

BAN SMOKING IN ALL PUBLIC PLACES/WORKPLACES, PENDING THE TOBACCO INDUSTRY PROVING, CONCLUSIVELY, THAT SECOND-HAND SMOKE IS HARMLESS!

Errol E. Povah

Competing interests: The only "competing interests" that exist on this matter are: Public/worker HEALTH vs. tobacco industry WEALTH. I'm a VOLUNTEER....fighting for the former!

4 Questions, 2 comments 19 May 2003

tOM Trottier,
None
Abacurial.com, K1R 7V8, Canada,
None

Send response to journal:
Re: 4 Questions, 2 comments

Email tOM Trottier, et al.:
mailto:tOM@Abacurial.com?subject=Re:+4+Questions,+2+comments

4 Questions, 2 comments

I am not a medical person or statistician, but I have the following questions

1. Did smoking spouses of the studied never-smokers live and smoke for the entire 39 year study period? If not, were never-smokers excluded for the period when they did not live with a smoker? Not excluding them would bias the study by including people not at risk from environmental smoke.

2. More generally, given that smokers' lungs cleanse themselves after several years of non-smoking, were the never-smokers excluded when this period lapsed after they were separated from their smoking spouses by separation, divorce, or death, or after their partner quit smoking

3. Given that the more smoked, the more danger to the smokers, did the never-smoking spouses of smokers who died from a smoking-related disease suffer an increased risk of dying from a smoking-related disease compared to never-smokers whose spouses did not die?

4. Were the never-smokers who lived with those who smoked 2+ packs per day at greater risk of smoking-related disease or mortality?

Comments

A. The study says, "It is also unclear how the reported increased risk of coronary heart disease due to environmental tobacco smoke could be so close to the increased risk due to active smoking (30% and 70%, respectively), since environmental tobacco smoke is much more dilute than actively inhaled smoke." ... "As it is generally considered that exposure to environmental tobacco smoke is roughly equivalent to smoking one cigarette per day,4 we extrapolated the relative risk due to exposure to environmental tobacco smoke from the relative risks for smoking 1-9 cigarettes per day. These extrapolated relative risks were about 1.03 for coronary heart disease and about 1.20 for lung cancer and chronic obstructive pulmonary disease. Based on these findings, exposure to environmental tobacco smoke could not plausibly cause a 30% increase in risk of coronary heart disease in this cohort, although a 20% increase in risk of lung cancer and chronic obstructive pulmonary disease could not be ruled out."

Shouldn't the researchers consider and report this risk in the conclusion, rather than cavalierly dismiss their data as implausible without even a reference?

The exposure time is longer with environmental smoke. There may also be effects due to the extended exposure time, or due to the accumulations of pollutants in the lungs over time.

This deserves further study, not dismissal.

B. Although the results are consistent with no risk within significance bounds, this is due to the small numbers of the study. A 50% (in women) or 100% (in men) increased death risk due to chronic obstructive pulmonary disease would not ordinarily be discovered by this study.

tOM Trottier, tOM@Abacurial.com

Competing interests: None declared

RESPONSE TO PAPER ON PASSIVE SMOKING 19 May 2003

Christopher W IDE,
Occupational Physician
25, Riverside Road, Waterfoot, EAGLESHAM, Glasgow G76 0DQ

Send response to journal:
Re: RESPONSE TO PAPER ON PASSIVE SMOKING

Email Christopher W IDE:
mailto:ide@lineone.net?subject=Re:+RESPONSE+TO+PAPER+ON+PASSIVE+SMOKING

Dear Sir/Madame,

I am concerned at the hysterical response to the paper in this week's BMJ which suggests that passive smoking may not be as harmful as previously thought. It is important to remember that the paper concentrated on mortality from heart disease, etc. Ever since 1970, there has been a steady fall of between one and three percent per year in mortality from ischaemic heart disease (IHD) so perhaps this would have some bearing on the results of the study - perhaps the authors - or someone else - might wish to compare the incidence IHD in spouses of smokers vs never smokers.

Lung cancer in 'never smokers' is a rare disease. Assuming that a large enough study population could be assembled, it would be interesting to test the hypothesis that one would expect even fewer cases of Ca lung in the 'never smokers', given that there has been a striking decline in the proportion of the population who smoke over the past three+ decades.

I remain,
Yours faithfully,
Christopher W Ide

Competing interests: None declared

A "passive smoke" observation 19 May 2003

Robert I. Rudolph, M.D., FACP,
Clinical Professor of Dermatology, University of Pennsylvania, Philadelphia, PA USA
1134 Penn Avenue, Wyomissing, PA 19610 USA

Send response to journal:
Re: A "passive smoke" observation

Email Robert I. Rudolph, M.D., FACP:
mailto:r-rudolph@comcast.net?subject=Re:+A+"passive+smoke"+observation

I'll let the illuminati and cerebri brawl over the scientific merits of the paper, but one conclusion is

absolutely unequivocal to this reader: passive smoke makes everyone - and everything - stink. I personally feel that anything so noisome probably causes something more than just immediate revulsion and reek!

Competing interests: None declared

Remember "Frank Statement to Smokers"? 19 May 2003

Joanne L. Addison,
oncology nurse
Newcastle NE39 2AJ

Send response to journal:
Re: Remember "Frank Statement to Smokers"?

Email Joanne L. Addison:
mailto:joanneaddison@lycos.com?subject=Re:+Remember+"Frank+Statement+to+Smokers"?

Although older than I am, the "Frank Statement to Smokers" I have read, issued by tobacco companies in 1954, created a non-existent controversy about smoking having a "causal relationship" with lung cancer and other serious disease, with any such allegations claimed to be "inconclusive".

Why do I have the feeling this is déjà vu all over again?

It's taken nearly 40 years for this particular orchestrated propaganda generated by the tobacco industry to run its course, and nobody of education questions anymore that smoking kills. Were it not for tobacco litigation proving otherwise, no doubt additional "studies" would surface at strategic intervals to "prove" that smoking is not necessarily the cause of death for literally millions of smokers worldwide.

With smoke-free legislation sweeping the globe, time to trot out a modern-day version of tobacco's agenda: this time, issuing a "study" that takes issue with the obvious. Tobacco smoke kills smokers, but nonsmokers exposed to it are, magically, not adversely affected.

BMJ, what have you done? This is not dispassionate journalism. It is journalistic naivete taken to the nth degree, which undermines the efforts of all medical professionals who actively work to improve quality of life and prevent preventable suffering.

Why do you claim there is a "controversy" when none exists? Wake up! The jury is not out - it is IN. Guilty. Of murder. With malice aforethought.

They may have fooled you; nobody else has fallen into their trap. How can we trust anything else you publish, when it appears you are so easily for sale?

Editorial Reference 1954 Frank Statement to Smokers: http://www.pmdocs.com/getimg.asp?pgno=0&start=0&bool=Frank%20Statement&docid=2015002376

Competing interests: None declared

Bully for the BMJ 19 May 2003

Bryce C. Peterson, M.D.,

Retired
80 Central Park West, NYC 10023

Send response to journal:
Re: Bully for the BMJ

Email Bryce C. Peterson, M.D.:
mailto:bcpeterson@doctor.com?subject=Re:+Bully+for+the+BMJ

Virtually everything I planned to say was said better by Dr. Gori.

Without researchers like Kabat and Enstrom, and journals willing to publish their unpopular results, ALL science is suspect because there are no checks and balances, only special interest zealots pushing an agenda.

I recently retired after forty years in general practice and it became very clear that the lunatics were in charge of the asylum on this issue nearly a decade ago.

Thanks to Professors Kabat and Enstrom and to the BMJ, maybe wiser, more reasonable heads will prevail.

B.C. Peterson, M.D., Retired NYC

Competing interests: None declared

Six Key Issues 19 May 2003

Ronald M. Davis,
Henry Ford Health System
One Ford Place, 5C; Detroit, Michigan 48202-3450; USA

Send response to journal:
Re: Six Key Issues

Email Ronald M. Davis:
mailto:rdavis1@hfhs.org?subject=Re:+Six+Key+Issues

There are several distinct issues at play surrounding the paper by Enstrom and Kabat. I would like to list many of them, and to add a few comments to those already appearing in the Rapid Responses to this paper.

1. QUALITY OF THE STUDY / VALIDITY OF THE DATA

Others have commented on this already.

2. ADEQUACY OF THE PEER AND EDITORIAL REVIEW OF THE PAPER

If one accepts the position that there are serious questions about the validity of the data reported in this paper, then it is appropriate to examine the adequacy of the peer and editorial review of the paper at the BMJ. BMJ editor Richard Smith has indicated in his Rapid Response that,

"Two top epidemiologists--including George Davey-Smith--reviewed the paper. Then the paper went to our hanging committee, which always includes a statistician as well as practising doctors and some of us."

A potential flaw in this process is that apparently, no one with special expertise in research on the health effects of passive smoking was involved in the review of this paper. I do not question the knowledge and abilities of the two top epidemiologists who reviewed the paper, or those of the in-house editors and the statistician on the "hanging committee." But in an area as complex as this -- to which massive reports have been devoted (1, 2) -- one or more persons with epidemiologic expertise AND an intimate knowledge of the literature on this subject should have been involved in the review of this paper. The obligation to find such a reviewer is heightened when one considers that the results of this study challenge a huge body of evidence in an area of enormous public health importance.

From my past editorial work at the BMJ (see my disclosure of competing interests), my impression has been that the BMJ's process of peer review includes more intensive INTERNAL review than other leading general medical journals, but less intensive EXTERNAL review. According to the BMJ's website, papers that are sent for external review (ie, those not immediately rejected) "are sent to one or more external reviewers" (http://bmj.com/advice/peer_review.shtml). Use of one external reviewer seemed to be the norm based on my experience at the BMJ. In a study involving the BMJ peer review process,(3) manuscripts were sent to two reviewers, suggesting that promising manuscripts at the BMJ are typically sent to NO MORE THAN two reviewers. Other journals routinely send promising papers to three external reviewers, or even more if the paper covers a complex or politically sensitive topic, or when serious conflicts of interest are present (as is the case here).

3. CONFLICTS OF INTEREST

Others have commented on this already.

4. WHETHER THE BMJ SHOULD PUBLISH STUDIES FUNDED BY THE TOBACCO INDUSTRY

BMJ editor Richard Smith explained the BMJ's position on this matter in his Rapid Response: "We long ago decided that we would not have a blanket policy of refusing to publish research funded by the tobacco industry, as some journals have done.* Our argument was that a ban would be antiscience, systematically distorting the scientific record."
* Roberts J, Smith R. Publishing research supported by the tobacco industry. BMJ 1996; 312: 133-134.

I simply want to point out that the BMJ position is explained further in a debate published after the 1996 article referenced above: King J, Yamey G, Smith R. For and against: Why journals should not publish articles funded by the tobacco industry. BMJ 2000; 321: 1074-6.

5. PUBLIC RELATIONS SPIN

Clive Bates expressed legitimate concerns about the impact of the BMJ's press release, even though he had not seen it. If he had seen the press release (which is available at http://bmj.com/content/vol326/issue7398/press_release.shtml#1), I suspect he would have become apoplectic. It looks like it had been written by the tobacco industry, stating that "This study will add to the already controversial debate on the health impact of passive smoking." After six paragraphs parroting the views of Enstrom and Kabat, the press release then states, "The impact of environmental tobacco smoke on health remains under dispute, writes Professor George Davey Smith in an accompanying editorial."

In its eight paragraphs, the press release allocates three words to the study's limitations: "Despite some limitations, this large study has several important strengths, add the authors...."

And the coup de grâce is that the press release does not mention the authors' conflicts of interest.

This problem is not unique to the BMJ. An analysis of press releases issued by seven medical journals (including the BMJ) included 23 studies that were industry funded; only 22% of the corresponding press releases revealed the source of funding.(4)

Compounding the press release is the cover of the journal, which ran the headline "Passive smoking may not kill." The same sentence is the caption for the photo-link to the "current issue" on the journal's home page this week (http://bmj.com).

6. EDITORIAL POSITION OF THE BMJ

A journal will confuse its readers, and the public at large, if the editorial positions it takes reverse directions without explanation. In 1997 the BMJ published two meta-analyses providing compelling evidence that passive smoking is a cause of lung cancer and ischemic heart disease.(5, 6) An accompanying editorial, which I authored, stated that these two meta-analyses and several other systematic reviews "make it clear that exposure to environmental tobacco smoke is a cause of lung cancer, heart disease, and other serious illnesses."(7)

A BMJ editorial published in 2002 (8) refers to "the fact that environmental tobacco smoke is estimated to be killing more than 1000 non- smokers in the United Kingdom each year," citing the 1998 "Report of the Scientific Committee on Tobacco or Health."

Now, the editorial position taken by Davey Smith is that: 1) "the impact of environmental tobacco smoke on health remains under dispute"; 2) "the considerable problems with measurement imprecision, confounding, and the small predicted excess risks limit the degree to which conventional observational epidemiology can address the effects of exposure to environmental tobacco smoke"; and 3) "Mendelian randomization ... is a promising strategy if we really want to know whether passive smoking increases the risk of various diseases."

BMJ editor Richard Smith, in his Rapid Response, casts further doubt on the link between passive smoking and disease when he states, "We are certainly interested in the question of whether passive soming (sic) kills, and it's clear to us that the question has not been definitively answered. Indeed, it may well never be answered definitively. It's a hard question, and our methods are inadequate."

The "Website of the Week" column in this issue of the BMJ (May 17, 2003) states, "While the connection between active smoking and disease has been widely documented, the jury is still out on the dangers of passive smoking, which is the subject of a paper and an editorial in this week's BMJ."

What accounts for the 180-degree change in the BMJ's editorial position on passive smoking between 1997 and now? Is this one study by Enstrom and Kabat, with its serious limitations, solely responsible for the BMJ's flip-flop? Where is the evidence that the results of this study (even if accepted at face value) will invalidate the results of the systematic reviews published by the BMJ in 1997 (5, 6) and other authoritative analyses of the literature (1, 2)? The editorial by Davey Smith does not answer those questions.

Ronald M. Davis, M.D.
Center for Health Promotion and Disease Prevention
Henry Ford Health System
Detroit, Michigan, USA

1. US Environmental Protection Agency. Respiratory health effects of passive smoking: lung cancer and other disorders. Washington, DC: EPA, 1992. (Publication EPA/600/6-90/006F.)

2. California Environmental Protection Agency, Office of Environmental Health Hazard Assessment. Health effects of exposure to environmental tobacco smoke. Sacramento: California Environmental Protection Agency, 1997.

3. van Rooyen S, Godlee F, Evans S, Black N, Smith R. Effect of open peer review on quality of reviews and on reviewers' recommendations: a randomized trial. BMJ 1999; 318: 23-7.

4. Woloshin S, Schwartz LM. Press releases: translating research into news. JAMA 2002: 287: 2856-8.

5. Law MR, Morris JK, Wald NJ. Environmental tobacco smoke exposure and ischaemic heart disease: an evaluation of the evidence. BMJ 1997; 315: 973-80.

6. Hackshaw AK, Law MR, Wald NJ. The accumulated evidence on lung cancer and environmental tobacco smoke. BMJ 1997; 315: 980-8.

7. Davis RM. Passive smoking: history repeats itself. BMJ 1997; 315:961-2.

8. West R. Banning smoking in the workplace. BMJ 2002; 325:174-5.

Competing interests: I have been active in tobacco research and tobacco control advocacy since 1979. From 1991 to 1998, I was editor of the journal "Tobacco Control," which is published by the BMJ Publishing Group. I served as North American editor of the BMJ from 1998 to 2001. I have testified as an expert witness in many tobacco-related lawsuits (including one devoted to passive smoking), but I have derived no personal income from such work (fees for my services have been paid to my employer, as I am a salaried employee).

The bottom line 19 May 2003

Andrew S Furber,
Specialist Registrar in Public Health
Eastern Wakefield Primary Care Trust, Castleford, WF10 5LT

Send response to journal:
Re: The bottom line

Email Andrew S Furber:
mailto:andrew.furber@ewpct.nhs.uk?subject=Re:+The+bottom+line

The publication of the article by Enstrom and Kabat [1] has aroused a storm of controversy, but in my view makes little difference to the key issue of whose rights are paramount – those of the smoker or those of the non-smoker.

Epidemiological research will nearly always contain an element of uncertainty and single studies need to be seen in the context of the whole body of research into the topic. This study, with all its strengths and weaknesses, is no exception. The strength of the effect and the technical difficulties in conducting this type of research may mean that demonstrating the health effects of second hand smoke at a population level is problematic.

However the bottom line is that tobacco smoke is known to contain more than 4,000 chemicals, many of which adversely affect health. I (as a non-smoker) do not want any exposure to me or my family to second hand smoke. Most smokers are very reasonable and, when asked, will respect this right and either stop smoking or smoke elsewhere. Governments need to act to ensure that asking becomes unnecessary, whilst allowing smokers to continue their habit without exposing others to

second hand smoke.

1. Enstrom JE, Kabat GC. Environmental tobacco smoke and tobacco related mortality in a prospective study of Californians, 1960-98. BMJ 2003;326:1057-60.

Competing interests: None declared

What killer? Let's call things with their name. 19 May 2003

Gian L. Turci,
C.E.O.
FORCES International

Send response to journal:
Re: What killer? Let's call things with their name.

Email Gian L. Turci:
mailto:gianturci@forces.org?subject=Re:+What+killer?+Let's+call+things+with+their+name.

The situation on passive smoke is quite simple. The heterogeneity of the studies militates against a formal meta-analysis, and the general and admitted weakness of results rather favors a simple eyeball appraisal. Out of a total of 123 studies (excluding this last one) 16 have shown a risk elevation for cancer, 30 have shown a benefit from exposure, all the rest failed to demonstrate either way. Out of the 16 studies mentioned above, NONE showed an elevation of risk greater than 20 percent. We all know that in this kind of epidemiology, it takes an odds ratio elevation of 200% or more just to demonstrate that a correlation EXISTS.

The US National Cancer Institute affirms that "Relative risks of less than 2 are considered small and are usually difficult to interpret. Such increases may be due to chance, statistical bias, or the effect of confounding factors [other causes] that are sometimes not evident"- and this is just an example.

Thus, the 16 studies could not even demonstrate unequivocally the existence of a correlation. The evidence for other diseases attributed to passive smoking is even weaker. What killer? Propaganda and instigation of hysteria (whether done by public institutions or otherwise) do not constitute proof or evidence -- unless, of course, we want to tell the truth and use the real names: intolerance and prohibitionism; but please do not call it scientific demonstration, for that insults science and intelligence!

Competing interests: None declared

environmental tobacco smoke paper requires further benefits from critical appraisal

19 May 2003

Rosemary Fox,
specialist registrar, public health medicine
national public health service wales, temple of peace and health , cardiff cf10 3nw,
Gwendolyn L. Lowe, Hugo van Woerden

Send response to journal:
Re: environmental tobacco smoke paper requires further benefits from critical appraisal

Email Rosemary Fox, et al.:
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subject=Re:+environmental+tobacco+smoke+paper+requires+further+benefits+from+critical+appraisal

The paper by Engstrom and Kabat (1) covers an important Public Health issue. Given the study's length and complexity, we used formal critical appraisal techniques to review the findings (2).

Although the study addressed a clearly defined and important issue, it was unclear why California alone was selected as the subset for analysis. Was it randomly chosen, or selected for a specific reason? If the latter, were attempts made to ensure it did not differ systematically from the full cohort?

While a cohort design initially appears appropriate to answer this question, the comparative rarity of outcome measures such as lung cancer in a non-smoking population suggests that a nested case control design may have been more appropriate. Despite the large cohort, the rarity of the outcomes used may have left this study under-powered to detect a significant difference between the groups.

The researchers are to be congratulated on achieving a 69% follow up after a forty year period. However, given the rarity of outcomes, even if only a handful of cases of lung cancer had occurred in those lost to follow up, the findings could have been very different.

There was a lack of clarity as to whether the assessors who assigned ICD codes to the deceased based on death certificate data were blinded to the exposure status of all cases.

The authors acknowledge that the smoking habits of spouses are a proxy measure of exposure to constituents of inhaled tobacco smoke. An exercise explicitly validating reported against actual exposure in a subsample would have strengthened the findings.

Overall, we found this paper difficult to interpret. The data tables were unhelpful, raising more questions than they answered. In the final analysis, we were unable to assure ourselves of the validity of the findings.

1. Enstrom JE, Kabat GC. Environmental tobacco smoke and tobacco related mortality in a prospective study of Californians, 1960-98. BMJ 2003; 326: 1057-61.

2. Health Evidence Bulletins- Wales: Questions to assist with critical appraisal of an observational study. Project Methodology 4. Cardiff: National Assembly for Wales, 2001. Available at: http://hebw.uwcm.ac.uk/projectmethod/appendix8.htm (accessed 19th May, 2003).

Competing interests: None declared

BMJ, Impact Factor and Irresponsible Journalism: A 'nasty' nexus?

19 May 2003

Zubair Kabir,
Research Fellow
CResT Directorate, St. James's Hospital, Dublin 8.

Send response to journal:
Re: BMJ, Impact Factor and Irresponsible Journalism: A 'nasty' nexus?

Email Zubair Kabir:
mailto:kabirz@tcd.ie?subject=Re:+BMJ,+Impact+Factor+and+Irresponsible+Journalism:+A+'nasty'+nexus?

Dear Editor,

The recent publication of Enstrom's et al study (1) is another piece of 'irresponsible journalism' on the part of one of the top general medical journals across the globe. This would not only stimulate further debate, as quite evident from the wealth of rapid responses published each day, but also wastage of resources on carrying out further studies. In doing so, this article stands a very fair chance of being cited more frequently over the next two years. Consequently, this may give an indirect boost to the relatively low 'impact factor' of the BMJ among the top five general medical journals (2). It would be interesting to conceive a hypothesis whether irresponsible journalism influences the impact factor of a journal! How to go about it?

There is no dearth of samples, but statistical power may be a factor. How about exposure misclassification bias? I do not think assessing 'irresponsible journalism' is that simple as assessing ETS! There may be similar issues, such as mainstream or side-stream second-hand smoke. Do we have a proxy measure for assessing our exposure of interest? For example, using 'responsible' journalism among other sister BMJ publications, such as Thorax or Heart as a surrogate, similar to smoking history of spouses in Enstrom's et al study (1).

What about the endpoint of our proposed hypothesis? Impact factor with its inherent limitations (2) may not be far worse than using 'overall lung cancer mortality' in assessing ETS exposure (1). I wonder if squamous -cell carcinoma of lung has a similar biologic mechanism to lung adenocarcinoma. If true, what may be the underlying risk factor for the rising 'epidemic' of lung adenocarcinoma, as opposed to declining squamous -cell carcinoma incidence worldwide (3).

The greatest limitation in any epidemiological study, however, is controlling for potential confounders. Funding may be a strong confounder, but can we eliminate 'residual' confounding at all? By contrast, 'competing interests' may be 'intermediate factors' in the causal pathway.

Finally, the study design for the hypothesis in question. Observational studies are among the lowest level of evidence. RCTs may not be ethically appropriate. Can we wait for our potential findings to be included in one of the future 'meta-analysis' for establishing any causal effect? I wonder if Bradford Hill's causality criteria (4) are living dinosaurs these days. Well, there is 'Mendelian randomisation' (5) to our rescue!

Nonetheless, the public-health implications of such findings may be huge: similar to Dr Wakefield's 'scare-mongering' paper on MMR in the Lancet in 1998, and the saga still continues.

It is incredible how tobacco industries push some 'responsible' scientists to 'intellectual sterility', despite millions of premature deaths worldwide!

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1. Enstrom JE, Kabat GC. Environmental tobacco smoke and tobacco related mortality in a prospective study of Californians, 1960-98. BMJ 2003; 326: 1057-61.

2. Joseph KS. Quality of impact factors of general medical journals. BMJ 2003; 326: 283.

3. Morita T. A statistical study of lung cancer in the annual of pathological autopsy cases in Japan from 1958 to 1997, with reference to time trends of lung cancer in the world. Jpn J Cancer Res 2002; 93: 15-23.

4. Hill AB. The envioronment and disease: association or causation? Proc. R. Soc. Med. 1965; 58: 295.

5. Davey Smith G. Ebrahim S. 'Mendelian randomization': can genetic epidemiology contribute to understanding environmental determinants of disease? Int J Epidemiol 2003; 32: 1-22.

Competing interests: May be once this eletter is published in print!

Adequacy of age-adjustment? 19 May 2003

Eugene Milne,
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Send response to journal:
Re: Adequacy of age-adjustment?

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For a few hours on 20 May, this letter included an additional note, at the author's request, which has since been removed. He has posted a later rapid response, in addition to this one.

With regard to Enstrom and Kabat's paper (1), I note that among the female 1959 never smokers, there was an inverse gradient of female age associated with spousal number of cigarettes consumed per day (Table 3: 1- 19 cigarettes vs mean age 53.7 years; 20-39 vs 50.9; >=40 vs 49.8).

I note also that there is an apparent trend in the direction of reduced risk of female CHD death with increasing spousal cigarette consumption (table 8). This trend was not abolished by age-adjustment or 'full adjustment' in the calculations of relative risk.

While it may be plausible that the risk of secondary smoking is less than has been claimed, it seems very highly implausible that one should see a trend of diminishing risk with apparent increasing exposure. Any confounding factor responsible for this effect would have to be consistently and quantitatively associated with increasing spousal smoking. It is hard to imagine what this factor might be.

Alternatively, the persistence of an inverse association between risk of CHD death in women and quanitity of spousal cigarettes smoked might more easily be explained by inadequate age-adjustment in the authors' calculations - for example, by using 5-year age banding rather than single years. Although this is not stated in the paper, the Method makes reference to a previous paper (2) in which standardisation was indeed done in 5-year age groups.

The authors have argued that by using cohort data they present more persuasive evidence of causality (or lack of it) than can be achieved through meta-analysis. Yet their results conjure a biologically implausible relationship which needs to be explained. In the absence of such an explanation, it remains most likely that their statistics are wrong.

1. Enstrom JE, Kabat GC. Environmental tobacco smoke and tobacco related mortality in a prospective study of Californians, 1960-98. BMJ 2003;326: 1057-61.

2. Enstrom JE, Heath CW Jr. Smoking cessation and mortality trends among 118,000 Californians, 1960-97. Epidemiology 1999;10: 500-12.

Competing interests: None declared

Response to Simon Chapman 19 May 2003

Michael J Thun,
Vice President, Epidemiology and Surveillance Research
American Cancer Society, 1599 Clifton Road, Atlanta, GA, 30309

Send response to journal:
Re: Response to Simon Chapman

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mailto:mthun@cancer.org?subject=Re:+Response+to+Simon+Chapman

(Addition to my comments submitted on May 17)

It is understandable that Simon Chapman had difficulty interpreting the information in the supplemental tables published by Enstrom and Kabat in the BMJ website. These tables are poorly labeled and are confusing, even for someone familiar with the study. In reality, the definition of exposure in the analysis is based solely on information about the smoking habits of the spouse collected in 1959, 1965, and 1972. The consideration of duration of exposure presented in Table 5 did not consider any updated information on smoking behavior by the spouse from 1973 to 1999, an interval representing two-thirds of the 39 year follow-up.

Nor should readers be reassured about the validity of the exposure measure by data from the resurvey of 681 subjects in 1999. Subjects who survived and provided information on smoking in 1999 comprised 7% of the original 9619 lifelong non-smokers at enrollment, and 15% of those followed after 1972. While Tables 2-6 on the website purport to present detailed information on characteristics of the 1999 respondents, most of the cells are empty and/or the percentages are based on fewer than five subjects.

The problem with misclassification of exposure is most critical with respect to coronary heart disease, since the risk of death from heart disease changes relatively quickly after cessation of exposure to tobacco smoke. In a separate analysis by tobacco industry consultants of coronary heart disease (CHD) in relation to environmental tobacco smoke (ETS) exposure in both CPS-I and CPS-II, LeVois and Layard obscured the association between ETS exposure and increased CHD mortality in CPS-II by emphasizing the results that combined subjects currently and formerly exposed and by stratifying currently exposed subjects so as to eliminate statistical significance (1). A reanalysis of the CPS-II data conducted by the American Cancer Society (ACS) revealed increased CHD mortality among non-smoking men and women married to current smokers compared to those whose spouses did not smoke (2). ACS did not attempt to replicate the LeVois and Layard analysis of CPS-I (1), because the CPS-I data were considered uninformative with respect to ETS and CHD because of the potential for misclassification of exposure. This point was made repeatedly in publications (3) and in communications with Drs. Enstrom, LeVois, and Layard. Despite the scientific flaws of that earlier study, however, the results have been widely publicized and misrepresented in the lay press whereever communities consider more stringent restrictions on public smoking.

To repeat, the analyses by Drs. Enstrom and Kabat are uninformative with respect to the adverse health effects of ETS exposure. Their report derives the appearance of legitimacy from being based on a small (10%) subset of an important ACS cohort. However, the enormous contribution that CPS-I has made to the scientific understanding of active smoking does not mean that the study is similarly informative regarding ETS exposure. Simon Chapman has asked if there are "obvious flaws" in the Enstrom and Kabat report. These flaws are inherent in the inadequacy of the information on ETS exposure and in the overinterpretation of the significance of these "negative" findings.

1) LeVois ME, Layard MW. Publication bias in the environmental tobacco smoke/coronary heart

disease epidemiologic literature. Reg Toxicol Pharmacol 1995;21:184-91.

2) Steenland K, Thun M, Lally C, Heath C. Environmental tobacco smoke and coronary heart disease in the American Cancer Society CPS-II cohort. Circulation 1996;94:622-8.

3) Thun M, Henley, J, Apicella L. Epidemiologic studies of fatal and nonfatal cardiovascular disease and ETS exposure from spousal smoking. Env Health persp 1999;107 Suppl 6:841-6.

Competing interests: None declared

Why The Double Standard? 19 May 2003

Dave Hitt,
None
None 12151 (home zip)

Send response to journal:
Re: Why The Double Standard?

Email Dave Hitt:
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When assessing the validity of a study, funding is one of many important factors to consider. Yet, I've never seen the anti-smoker crowd question the funding of studies that support their position. Anti-smoking groups, including Big Pharma, who makes enormous profits selling aids to quit smoking, are almost always the ones providing the funding. Why aren't *their* agendas questioned? Why the double standard?

I sent this question to Joe Cherner, one of the most active leaders of the anti-smoker movement, and he refused to answer, instead sending replies like "Who are you?"

This study meshes rather nicely with the 1998 study funded by the World Health Organization. Their methodology was a textbook example of the right way to conduct a case-control study, yet, to their horror, they found no statistically significant increase in lung cancer due to SHS. They did their best to bury the report, and would have succeeded if not for the hounding of the British press.

Why have so many physicians forgotten the basic rule of toxicology: *The Dose Is The Poison?* When Covance Laboratories measured ETS exposure, using portable air pumps worn by non-smokers in smoky environments, they discovered the exposure was six cigarettes *per year*. Studies by the Oak Ridge Laboratories yielded similar results. These were actual measurements, not statistical number juggling. Smokers have to smoke 14,000 – 35,000 cigarettes per year for a decade, and sometimes two or three decades, before the ill effects set in. With that in mind, is it biologically plausible that the smoke from six cigarettes per year kill a quarter as many people as primary smoking?

Some people may find second hand smoke irritating, annoying, or obnoxious, but it's no more dangerous than sitting downwind from a campfire. It's about time some real science was applied to this subject, instead of the hysterical hand wringing over studies funded by groups with agendas that are just as obvious as big tobacco's agendas, at least to anyone who takes a moment to consider them.

Competing interests: The author is a member of CLASH, Citizens Lobbying Against Smoker Harassment, and maintains the web site The Facts, http://www.davehitt.com/facts/

Pots calling kettles black 19 May 2003

GH Hall,
Retired Physician
EX1 2HW

Send response to journal:
Re: Pots calling kettles black

Email GH Hall:
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Why was the grant from the Tobacco-Related Disease Research Program terminated? Was it because the finding of no excess tobacco related diseases in non smoking spouses was accepted, and hence further support would be superfluous? Or was it because these findings were contrary to the dogma of the anti-smoking lobby? If that were the case, then accusations of suppressio veri, suggestio falsi, so often issued against tobacco industry supported workers have rebounded, at least in regard to the matter of trying to conceal evidence unfavourable to one's case. The bias of the commentator- no recognition of the virtues of this study, with much carping criticism makes one despair of getting impartial views. In this field, as in others, truth is not obtained by the flawed philosophy of Ends justifying Means.

Competing interests: None declared

SARS causes no harm 20 May 2003

Jonathan P. Krueger,
privacy capacity
94566

Send response to journal:
Re: SARS causes no harm

Email Jonathan P. Krueger:
mailto:jpk@rawbw.com?subject=Re:+SARS+causes+no+harm

What a shame that SARS can't fund "studies" that claim to find "no increase in risk" for people exposed to it! Surely it would be possible to run a "study" that quietly neglected to mention a significant exposure to SARS in the control group.

SARS is a pathogen not greatly advanced in its lobbying, litigation, and public relations efforts. It doesn't seek out sympathetic scientists and pay for their research. It doesn't hide behind front organizations. It doesn't pay scientists to put their name behind its PR.

Tobacco by contrast is highly advanced. It does all of these things and more. For 50 years it paid to "keep doubt alive" about the effects of active smoking. Today it continues the fight to keep doubt alive about passive smoking.

As others have already indicated, and as has been brought out elsewhere, the authors of this study have failed to disclose the depth and breadth of their connections to the tobacco industry. Taken as a whole, there is no doubt that they have conflicts of interest the render the entire "study" without credibility of any sort. And people who find this out are generally aware that this is just part of the large, sustained, massively funded campaign by the tobacco industry to "keep doubt alive".

However most people won't find this out. As the industry is already demonstrating, this "result" will be pumped throughout the globe in industry PR, in the mouths of its front organizations, as "controversy" over passive smoking. The same as this industry did for 50 years with "the smoking and health controversy".

In the interests of fairness, I would appreciate it if BMG would kindly publish studies that purport to show that SARS and HIV and smallpox "cause no harm". If you can't find good studies that show this, that should be no obstacle; just publish the poor ones, as you did here.

I recognize the difficulty that no one will come to you with well funded studies showing this. That's just the natural result of the fact that no one's making over US$100 billion a year on a product that causes the spread of SARS and HIV. As someone is with tobacco.

Competing interests: None declared

Reply to ASH - please be more careful in future 20 May 2003

James A Delphi,
NA
Manchester, UK

Send response to journal:
Re: Reply to ASH - please be more careful in future

Email James A Delphi:
mailto:jimdelphi@hotmail.com?subject=Re:+Reply+to+ASH++-+please+be+more+careful+in+future

Deborah Arnott of ASH (Action on Smoking and Health) says:

"This is why ASH believes that the BMJ were wrong in publishing this article without making clear the tobacco industry connection"

However, the article contains the following statement at the end:

"Competing interests: In recent years JEE has received funds originating from the tobacco industry for his tobacco related epidemiological research because it has been impossible for him to obtain equivalent funds from other sources. GCK never received funds originating from the tobacco industry until last year, when he conducted an epidemiological review for a law firm which has several tobacco companies as clients. He has served as a consultant to the University of California at Los Angeles for this paper. JEE and GCK have no other competing interests. They are both lifelong non-smokers whose primary interest is an accurate determination of the health effects of tobacco.

The question is not about whether the competing interests were disclosed, but whether the BMJ was right to publish this work even if they were disclosed. That's a matter of weighing whether the safeguards are adequate to deal with probable bias.

The golden rule of criticising the work of others is not to destroy your own credibility in the first couple of lines of your contribution.

James Delphi

Competing interests: None declared

Inverse effect can be explained 20 May 2003

Wiel Maessen,
Board member of Forces International
Netherlands

Send response to journal:
Re: Inverse effect can be explained

Email Wiel Maessen:
mailto:wiel@forces-nl.org?subject=Re:+Inverse+effect+can+be+explained

Several authors have questioned how an positive effect of ETS can be explained.

Well, there is one explanation called the 'Hygiene Hypothesis': because we have become this hygienic - and banning ETS from the environment is such a measure - the immune system of children is not triggered sufficiently anymore during their crucial years.

The 1998 WHO/IARC study also found such a positive effect for children in connection to lung cancer. Many other studies also revealed this. And how could one explain otherwise that asthma rates have increased although smoking rates lowered significantly?

It seems that we are really thinking that we, as human beings, are able to protect our health better than nature itself. If we keep hiding ourselves from natural triggers, mankind might as well end up in an incubator within 100 years. Medicine will prosper though...

Competing interests: None declared

Freedom 20 May 2003

Crystal L Pherson,
Responsible Adult
San Antonio, Texas

Send response to journal:
Re: Freedom

Email Crystal L Pherson:
mailto:crysnyx@aol.com?subject=Re:+Freedom

Thank you for proving and publishing what I've known to be true for years. Personally, I am a smoker. I began smoking when I was 19 - when many members of my family had finally quit smoking. I grew up in a smoker's household & while I prefer not to smoke around children, that wasn't a concern for my mother. I shudder to think that if I were growing up in the same household today, my mother could have been taken away from me because she chose to smoke. I'm proud to say that neither I nor my sister ever suffered any "passive-smoking-related" illnesses in our childhood. Two women in my family died from diseases that may have been smoke-related; however, I respect their choices. Actually, my grandmother died within a year of quitting - her doctor admitted that had she never quit, it is likely she never would have been diagnosed with lung cancer.

In response to the tobacco litigation, I have personally extracted a promise from those around me to never hold a company responsible for this *person's* choice. I think it's about time that we allow ALL people to make their own choices & to remind those same people that we are all

responsible for the consequences of our choices. If, someday, I die from some so-called "smoke-related" disease, I know that it was my choice to smoke and that my life has been eminently enriched by the experiences I could only have known by being a smoker. Thank you for your time.

Competing interests: None declared

A very interesting coincidence 20 May 2003

ELIF DAGLI,
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Marmara University Istanbul Turkey

Send response to journal:
Re: A very interesting coincidence

Email ELIF DAGLI:
mailto:edagli@superonline.com?subject=Re:+A+very+interesting+coincidence

One of the most widely read newspapers of Turkey printed a news article on this paper on 17th May 2003. This is an extremely rare occasion for a Turkish newspaper which are renouned not to be prompt with health news.Coincidentally the owner of the newspaper had wedded her daughter to the son of of a family that is the Turkish partner of a multinational tobacco industry . Here is the webadress of the news http://www.milliyet.com.tr/2003/05/17/yasam/yas01.html

Prof Dr Elif Dagli

Competing interests: None declared

Confirmation: Secondhand smoke does cause respiratory disease

20 May 2003

Anthony J Hedley,
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Tai-Hing Lam, Sarah M. McGhee, Gabriel M. Leung, and Megan Pow

Send response to journal:
Re: Confirmation: Secondhand smoke does cause respiratory disease

Email Anthony J Hedley, et al.:
mailto:commed@hkucc.hku.hk?subject=Re:+Confirmation:+Secondhand+smoke+does+cause+respiratory+disease

Dear Dr Smith,

The report by Enstrom and Kabat1 confirms that exposure to secondhand smoke (SHS) causes injury to the respiratory system with the finding of a combined increased mortality risk for men and women for chronic obstructive pulmonary disease (Relative Risk 1.65 [1.0-2.73].2 This is consistent with other investigations which demonstrate respiratory system sensitivity to SHS at all ages and in different settings. In Hong Kong several studies have shown that the exposure of infants to SHS in utero or postnatally in the home was linked to higher consultation rates and hospitalization for respiratory and other illnesses.3 Smoking in the home was clearly associated with bronchitic symptoms in a cohort of primary school children, independently of ambient air pollution.4 In an adult workforce, workplace exposures to passive smoking were associated with significant excess risks (66% to 212%) for all respiratory symptoms5 and increased health care

costs.6 In a population survey the prevalence of SHS exposures at work was 47.5% among non-smoking full time workers compared with only 26% at home. Those exposed at work were 37% more likely to consult a doctor for respiratory illness. The increased health care costs for primary care alone among three million workers was estimated at US$29M annually.7 Four independent case control studies on lung cancer and passive smoking in Hong Kong, reviewed by the United States Environmental Protection Agency, gave an overall relative risk of 1.48 (1.21-1.81).8

In other words, we have epidemics of respiratory disease in Hong Kong caused by secondhand smoke. However because of the way in which the Enstrom and Kabat paper was presented and abstracted little or no attention will be paid in media reports to the findings on mortality risks from respiratory disease.

We agree with many of your correspondents that the reason why Enstrom and Kabat did not find any evidence of increased mortality risk from cardiovascular disease and lung cancer requires further explanation. We also agree with McKee9 that the authors' relationship with the tobacco industry (for example10,11), including the way in which they solicited the funds, as recorded in tobacco industry documents, should be clarified. The Wall Street Journal12 quotes Dr Enstrom as having stated in a letter to a Philip Morris executive in 1977, that

"a substantial research commitment on your part is necessary in order for me to effectively compete against the large mountain of epidemiologic data and opinion that already exists regarding the health effects of (ETS) and active smoking".

Can Dr Enstrom interpret this statement for us? This is not the way in which most of us compete for research funding.

Many observers of the tobacco industry's actions to undermine public health policies will be concerned as to whether the BMJ has made an adequate assessment of the provenance of this report including the possible implications of funding contributions by the Centre for Indoor Air Research.

On June 17 1988 the tobacco industry "Industry Interface Meeting on ETS" was held at the St James Court Hotel in London.13 The stated objective of the meeting was "to bring together industry scientists to discuss scientific research and strategies on ETS and how these relate globally". One of the aims expressed by some speakers was to create "marketable science". Dr Thomas Osdene of Philip Morris outlined the activities of the Centre for Indoor Air Research (CIAR). It was stated that

"He noted that although the tobacco industry had been active on ETS issues in the past, primarily through [the Tobacco Institute] a change in focus was required. As a result Philip Morris, Lorillard and RJ Reynolds formed CIAR to facilitate and support new research on indoor air quality generally, not just ETS. Likely topics include air quality in airline cabins and in the workplace. In addition to new research CIAR oversees ongoing research that was once the responsibility of the ETS Advisory Group".

In Hong Kong we are well aware of the CIAR's role and activities and those who have taken its funds. In the tobacco industry's Asia ETS Consultant Project14 it was stated that

"one key objective ... has been to recruit and educate (sic) scientists who would then be available to testify on ETS in legislative, regulatory or litigation proceedings in Asia or elsewhere".

The Hong Kong scientists (or "whitecoats" as the industry referred to them) were funded by the CIAR but were recruited to the consultancies by a representative of Philip Morris' lawyers, Covington and Burling. Having said that, although the stated source of funding for the Enstrom and

Kabat paper is the CIAR (which purports to have an independent review panel), Dr Enstrom also received funds directly from Dr Richard Carchman, Group Director Scientific Affairs, of the Philip Morris Tobacco Company.15,16

In the circumstances, the editorial office of the BMJ has taken a bold step in publishing a tobacco industry funded report. As the editor implies, its aim is to uphold academic freedom and intellectual honesty in all matters. The question is whether all the relevant facts have been adequately considered, because this single publication may have a profound effect on public health policy.

On our own turf it is likely that the paper by Enstrom and Kabat will be used by the Hong Kong Tobacco Institute to attempt to derail the Hong Kong SAR Government's forthcoming legislative proposals on smoke-free workplaces, including the catering and hospitality industry. Around the world the public health sector will undoubtedly have a bigger fight on its hands than it ever anticipated, because of the reported results of the study. The tobacco industry will spend whatever it takes to exploit the conclusion that secondhand smoke is "safe". As a result it is quite possible that smoke-free policies in many countries will be set back by several years; the effects of that will be measureable in terms of illness episodes, hospital admissions and premature deaths.

May we ask whether the BMJ will now disclose what specific steps it took to ensure that work funded by the tobacco industry was not subject to the kind of bias which has been clearly demonstrated in other industry sponsored publications17? If such an enquiry has not already been done then we suggest an investigation should be implemented immediately and if the outcome is unsatisfactory then the BMJ should take adequate steps to publish and disseminate the findings.

The whole issue of conflict of interest is fraught with problems of interpretation. Dr Gio Gori gives a spirited defence of your decision to publish ("Bully for the BMJ ..."). 18 He admits to "occasional support from the tobacco industry". This presumably includes the invoice to Covington and Burling for US$25000 on September 15 1992,19 but should we know how many more?

McKee10 points out that some declared conflicts of interest are invalid and lead to unproductive exchanges with the authors. There should be a mechanism for withdrawing papers for which conflicts of interest were not adequately declared. We suggest that, in future, declarations of possible conflicts of interest by authors should, if necessary, be based on very specific questions relating to professional or working associations with others who are involved with the tobacco industry and certified statements of personal consultancy incomes. Otherwise the medical literature on this and other issues is at risk of being corrupted.

AJ Hedley, Professor in Community Medicine
TH Lam, Professor and Head of Department
SM McGhee, Associate Professor
GM Leung, Assistant Professor
M Pow, Research Assistant
Department of Community Medicine, University of Hong Kong
Email: commed@hkucc.hku.hk

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1. Enstrom JE, Kabat GC. Environmental tobacco smoke and tobacco related mortality in a prospective study of Californians, 1960-98. BMJ 2003; 326:1057.

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13. http://www.smokescreen.org/documents. PM 23706 (Bates: Z021548222 [pm02,\PFIN 0002 \08]; Smokescreen ID: 133955).

14. http://www.smokescreen.org/documents. PM 26839 (Bates: 2500048976 [pm04,\PFIN004/14]; Smokescreen ID 164571).

15. http://www.pmdocs.com. (Bates No.: 2063610867)

16. http://www.pmdocs.com. (Bates No.: 2063654067)

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18. Gori GB. http://bmj.com/cgi/eletters/326/7398/1057 (18 May 2003).

19. http://www.pmdocs.com. (Bates No.:2025473903)

Competing interests: AJH is a former chairman of the Hong Kong Council on Smoking and Health (COSH). THL is current vice chairman of COSH. All of the authors conduct research on the health effects of active and passive smoking and have received research funds, through their employer, (University of Hong Kong) to support their work.

Re: Adequacy of age-adjustment - a hypothesis 20 May 2003

Eugene Milne,
Deputy Medical Director
Northumberland, Tyne & Wear Strategic Health Authority, Newcastle upon Tyne, NE4 6BE

Send response to journal:
Re: Re: Adequacy of age-adjustment - a hypothesis

Email Eugene Milne:
mailto:eugene.milne@ntwha.nhs.uk?subject=Re:+Re:+Adequacy+of+age-adjustment+-+a+hypothesis

Some further thoughts on the question of age-adjustment and a hypothesis: Suppose that the base populations were biased by random selection of couples. Heavy smokers die young, so the sub-populations of couples of whom one is a heavy smoker tend to be younger than couples of whom one is a light or non-smoker. Random sampling will thus tend to produce a younger population of heavy smokers than light smokers. When adjusting for age in the study, all couples of whom one was a smoker were treated as a single group. As a result, the adjustment for heavy smoking couples was less than it should have been, and the greater the degree of smoking the more exaggerated was the under-adjustment for that sub-population. Because the curve of CHD age-specific mortality is always exponential, the effect of under-adjustment was amplified significantly. The internal age structure of the 5-year bands used in adjustment might also have contributed to this effect - notably where there is a steep gradient of diminishing sample population size within the age band.

The net result would be that age-adjustment undertaken in good faith would produce progressively more inadequate adjustment as the smoking level of the spouse increased. Thus, an inverse relationship between number of cigarettes smoked and CHD mortality could arise purely through sampling and statistical error.

Competing interests: None declared

BMA Turns Tabloid 20 May 2003

Dale Jackaman,
Seriously Annoyed
I won't dignify this rag with my credentials

Send response to journal:
Re: BMA Turns Tabloid

Email Dale Jackaman:
mailto:dalej@reboot.bc.ca?subject=Re:+BMA+Turns+Tabloid

This should have been the BMJ's response to Enstrom et al.

http://www.pmdocs.com/getimg.asp?pgno=0&start=0&docid=2065122062

Thanks for turning back the clock on public health decades or more. We don't need this kind of negligence from what used to be a professional medical publication.

I seriously wonder who got paid off at BMJ to publish this utter garbage.

Competing interests: None declared

California-smoky offices, windows open at home most of the year ....

20 May 2003

HS Roberts,
NA
NA

Send response to journal:
Re: California-smoky offices, windows open at home most of the year ....

Email HS Roberts:
mailto:hank@spamcop.net?subject=Re:+California-smoky+offices,+windows+open+at+home+most+of+the+year+....

Riiiight.

I moved to California in 1976. For a great many years, I had to breathe tobacco smoke at work -- where the windows didn't open and the air was recirculated. Homes, even relatively new ones in California, are rarely much insulated, leak air like sieves when it's breezy, and usually have windows open much of the year.

A smoking spouse can be got upwind of. A business partner, enjoying his daily after-lunch cigar with his door open next to my little cubicle, was unavoidable for fifteen years in a then modern, well-sealed glass box building.

Show me data from somewhere that had restrictions on public smoking, and homes weatherized for winter conditions, and I'll believe that they're going to say something useful about the effect of living with a smoking spouse.

Show me who paid for the work- and - who paid the reviewers who say it's decent work, and I'll know how much to believe the data have been honestly analyzed.

The kind of lawyers who have long represented the lead industry -- these many decades during which the European nations restricted lead use and the United States was baffled and blinded by bovine byproduct -- also represent the tobacco companies, which can buy health researchers as they bought the American Civil Liberties Union to confuse what's good for human beings with what's good for these "corporate citizens of the world."

Competing interests: None declared

Re: Re: Nothing new from the antismoking front

20 May 2003

Wiel M Maessen,
Board member of Forces International
Netherlands

Send response to journal:
Re: Re: Re: Nothing new from the antismoking front

Email Wiel M Maessen:
mailto:wiel@forces-nl.org?subject=Re:+Re:+Re:+Nothing+new+from+the+antismoking+front

Mr. Copeland says: "How ironic that the BMJ accepts without comment a letter from someone who declares "no competing interests" but is a "Board member of Forces International"? Does no one at the BMJ know anything about this tobacco-industry advocacy group? Is it possible that you are so

naive you do not understand how sophisticated the tobacco industry's advocates are?

I challenge him to proof that Forces International has only ONE link to the tobacco industry. Forces International represents a third party in the War On Tobacco that is often forgotten: the smokers themselves. Smokers are crushed between the two industries on the nicotine market: the health industry and the tobacco industry.

The 'enlightened' health advocates like Mr. Copeland obviously are not aware that what they are doing to a large minority of people resembles the oppression we've seen here in Europe in the past. Their strategy is what is called 'Verelendung', oppressing people that hard that they don't resist anymore to a higher power. It's psychological warfare and the ETS lie is one of the main weapons in that war.

Smokers are pretty defenseless: they are pressed to finance their own oppression. They have to finance their own armies with donations where the anti-tobacco advocates are openly financed by the pharmaceutical industry and don't even know where to spend their money.

It's about time that the health industry advocates read some books on psychology and ethics. With their one-dimensional, physical health view on mankind they are blind for all the negative social en psychological damage they cause in society and to a large group of individuals.

It is well known in psychology that a people that is oppressed, generates mental illnesses like depression and anxiety related diseases. Some recent studies already show that depression is more common in smokers than in other people. What you anti-smokers earn on the physical side of health may well be causing sincere negative effects on the mental side.....

If I have to state here that I have conflicting interests, why don't you, Mr. Copeland, as a member of the health industry, fill in that field too? There's no objectivity in your view. Not more than in mine.

If you repeat your allegation that Forces International has any relationship with the Tobacco industry - other than as consumers of legal products of that industry - we will consider taking legal action against you.

Competing interests: None declared

Another "competing interest" of James Enstrom 20 May 2003

▲▼ Robert S. Broughton,
Computer Programmer
New Westminster, BC, Canada V3M 3N2

Send response to journal:
Re: Another "competing interest" of James Enstrom

Email Robert S. Broughton:
mailto:bob@broughton.ca?subject=Re:+Another+"competing+interest"+of+James+Enstrom

See http://www.pmdocs.com/getimg.asp?pgno=0&start=0&docid=2063610840/0841

It's a letter written by James Enstrom, and includes the following: "For the past three years I have done consulting and research on passive smoking... on behalf of R.J. Reynolds and Philip Morris".

Competing interests: None declared

More details on competing interests 20 May 2003

Martin McKee,
Professor of European Public Health
LSHTM, WC1E 7HT, UK,
Pascal Diethelm

Send response to journal:
Re: More details on competing interests

Email Martin McKee, et al.:
mailto:martin.mckee@lshtm.ac.uk?subject=Re:+More+details+on+competing+interests

Dear editor,

In an earlier rapid response, one of us called for clarification of certain aspects of Enstrom and Kabat's statement of competing interests. We are grateful to Professor Kabat for his clarification of involvement in an earlier paper with Professor Rylander [1] who, as we noted, has been shown to be a leading consultant for the tobacco industry [2].

However, in the light of the concerns that have been raised, it would also be helpful to be reassured that Professor Kabat was not involved in the work at the American Health Foundation funded by Philip Morris until 1991. This funding stream then funding ceased but was, however, followed by a substantial grant from Kraft, a member of the same industrial group as Philip Morris [3]. Interestingly, the letter from Kraft announcing the funding was copied to Steven Parrish, a senior executive at Philip Morris who has been active in their work on environmental tobacco smoke (ETS).

We also requested information on the nature of the funding awarded by CIAR, given that it funded two types of research, peer-reviewed and "special-reviewed", with the latter awarded directly by tobacco industry executives. As Barnes and Bero have shown [4], the special-reviewed projects were more likely than peer-reviewed projects to support the tobacco industry position and be used by the industry to argue against smoking bans in public places.

While awaiting a response we have undertaken our own research on the role played by CIAR using publicly available tobacco industry documents.

We note that, on 15 July 1996, Dr Enstrom, wrote to Dr Max Eisenberg, Executive Director of the CIAR, to introduce himself, stating that, "for the past three years I have done consulting and research on passive smoking for Jeffrey L. Furr of Womble Carlyle on behalf of RJ Reynolds and Philip Morris [5]." Womble Carlyle Sandridge & Rice is a law firm based in Winston-Salem, North Carolina, that was extensively involved on behalf of the tobacco industry in litigation in respect of the hazards of exposure to tobacco smoke indoors and Dr Enstrom had written reports on this topic for RJ Reynolds' legal department [6].

This was followed by a formal proposal to CIAR. The initial proposal, dated 28th June 1996 [7], set out a package of work that included reanalysis of three existing cohorts, including the CPS I study. The others were the NHANES I Epidemiologic Follow-up Study and the US Veterans Study.

We have obtained anonymous reviews which make it clear that the proposal was not highly regarded. One, by Dr J Oey, working at INBIFO, a facility in Germany owned by Philip Morris, noted that "The projects proposed are very large and diverse ... for all these projects/studies the hypotheses or objectives are not stated" and "procedures are indeed very general and do not help one in determining the quality of the proposal" [8]. Another reviewer noted that such a reanalysis would "only add to the increasing number of imperfect studies" [9].

Despite these somewhat sceptical reviews, CIAR appears to have been willing to proceed further. The minutes of the meeting of the Board of Directors of the CIAR, on 15th May 1997 record that Dr Max Eisenberg, Executive Director of the CIAR, stated that he was in separate discussions with Drs Enstrom and Kabat about "the possibility of their collaboration" [10].

Richard Carchman, Vice President (Scientific Affairs) of Philip Morris then wrote to Dr Enstrom to confirm a meeting that would also be attended by Dr Eisenberg in Los Angeles, on 19th September 1997 [11]. Carchman had previous contact with Dr Enstrom, for example when he wrote on 25th April 1997 to notify him that he had been awarded a grant of $150,000 by Philip Morris for research on "The relationship of low levels of active smoking to mortality" [12]. In his letter of 15th January accompanying that proposal [13] Dr Enstrom noted that "A level of trust must be developed based on my past research on passive smoking ... A substantial research commitment on your part is necessary in order for me to effectively compete against the large mountain of epidemiologic data and opinions that already exist regarding the health effects of ETS and passive smoking". Charles Green, of RJ Reynolds, and convenor of the industry-wide ETS Task Force, and who had taken a leading role in the industry campaign to undermine other work on the link between passive smoking and disease, was to have attended the meeting but was unable to. However Green did write to set up a follow-up meeting with Carchman on 9th October [14].

Subsequently, an amended proposal, dated 20th October 1997, now jointly with Dr Kabat and with Drs Lawrence Garfinkel and Clark Heath, from the American Cancer Society, was entitled "Environmental tobacco smoke and mortality among CPS I" [15].

Again, a review was unfavourable [16], noting:
- "the proposal appears to have multiple, mixed goals"
- "the proposal fails to distinguish between "ETS exposure" and "living with a spouse who smokes, makes light of the substantial loss-to-follow-up expected in this cohort, freely assumes adequate adjustment can be made for the non-representativeness of the initial cohort, underplays the import of smoking cessation on the analyses, and is uncritical of positions established by the non-smoking community"
- "the proposal fails to clearly define the analyses planned and hypotheses to be tested for ETS exposure"
- "the proposal fails to clearly define the data upon which the planned analyses will be based."

However the reviewer does note that "The list of consulting authors (Heath, Kabat, Garfinkel) is impressive and should add credibility to interpretation of results".

A memorandum from Dr Eisenberg, dated 3rd November 1997, then makes clear that Dr Enstrom's "proposed research on passive smoking" was to be considered by CIAR under its directed studies (or special reviewed) programme [17], which has elsewhere been found to mean that it would be undertaken under industry direction. Its special status seems to be confirmed in the minutes of the meeting of CIAR's Board of Directors on 19/20th November 1997. This listed 20 proposals that had been recommended for consideration by CIAR's Scientific Advisory Board. Dr Enstrom's proposal was taken separately and it was decided to fund it with (unspecified) modifications [18].

Since then, Dr Enstrom appears to have maintained his links with the tobacco industry, for example, speaking at a symposia and workshops, with tobacco industry representatives nominating him as an "appropriate" (sic) expert, along with others who are well known to be consultants to the industry [19]. These links extended to a meeting organised by Philip Morris staff in June 2000 with Peter Lee, a leading consultant to the tobacco industry, to "discuss the results of the CPS I and CPS II studies and develop possible approaches to analysing the data" [20].

Dr Enstrom reported that "In recent years [he] has received funds originating from the tobacco industry for his tobacco related epidemiological research because it has been impossible for him to obtain equivalent funds from other sources." Given the controversy that this paper has generated

we hope that this account will clarify the nature of this relationship.

Martin McKee
London School of Hygiene and Tropical Medicine, London, UK

Pascal Diethelm
OxyGenève, Geneva, Switzerland

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2 http://www.prevention.ch /rylanderpm.htm [accessed 14 May 2003]

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4 Barnes DE, Bero LA. Industry-funded research and conflict of interest: an analysis of research sponsored by the tobacco industry through the Center for Indoor Air Research. J Health Polit Policy Law 1996; 21: 515-42.

5 Enstrom JE. Proposed research on passive smoking. 15 July 1996. Bates Number: 2502317161; Master Document Id Range: 2502317161/7179.

6 Enstrom, J.E. "Report Concerning Smoking and Health Issues Prepared by Consultants Engaged by RJR for the Purpose of Assisting Attorneys in Connection with Ongoing Litigation Transmitted to RJR in-House Legal Counsel.". 01 Nov 1992. Bates: 515814425-515814435.

7 Enstrom JE. Proposed research on passive smoking. Bates Number: 2063610842/0847; Master Document Id Range: 2063610840/0847.

8 Oey J. Documentation P 0268/2195 Comments on: "Proposed research on passive smoking". Bates Number: 2505100107/0112; Master Document Id Range: 2505100106/0112.

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16 Comments on proposal, environmental tobacco smoke and mortality among CPS I. Undated. Bates Number: 566943526.

17 Eisenberg M. CIAR PROPOSALS. Dated 3 Nov 1997. Bates Number: 2076979214; Master Document Id Range: 2076979214/9270.

18 Eisenberg M. Center for Indoor Air Research. Board of Directors' meeting. 19/20 November 1997. Bates Number: 522077990/7993

19 Carchman R. Letter to B Rikert. 7 May 1998.

20 Sanders T. Tobacco smoke - lung cancer - histological types and analysis of major databases. Bates Number: 2505540920

Competing interests: As editor of the European Journal of Public Health, MM published another paper by authors funded by the Center for Indoor Air Research. This publication was the centre of a long-running dispute between the journal and the authors concerning undeclared conflicts of interest. It led to his involvement as a witness in a lengthy legal dispute that has recently been resolved. PD successfully defended a charge of libel in the same case. OxyGenève is an organisation involved in tobacco control. Both have received funding for work on tobacco control.

Tobacco cartel wants to create a "controversy" 20 May 2003

Joseph Cherner,
President
SmokeFree Educational Services, Inc., 10280

Send response to journal:
Re: Tobacco cartel wants to create a "controversy"

Email Joseph Cherner:
mailto:Joe@smokefree.org?subject=Re:+Tobacco+cartel+wants+to+create+a+"controversy"

When you read the formerly secret documents of the tobacco cartel (released as part of the MSA), it is obvious that Big Tobacco's goal is to create and maintain "controversy" over whether tobacco smoke causes disease.

The tobacco cartel succeeded in maintaining controversy over the health effects of primary tobacco smoke for 30 years and millions of deaths. Finally, I think we have gotten passed the controversy stage.

Now the tobacco cartel continues to attempt to maintain controversy over the effects of secondhand smoke (even though its own websites admit the opposite). How long and how many deaths later will this "controversy" continue? Frankly, I thought we had gotten passed it, and I am sad to see the BMJ participate in it.

Competing interests: None declared

An American Cancer Society Perspective 20 May 2003

Michael J. Thun,
Vice President, Epidemiology and Surveillance Research
American Cancer Society, 1599 Clifton Road, Atlanta, GA 30329-4251mn

Send response to journal:
Re: An American Cancer Society Perspective

Email Michael J. Thun:
mailto:mthun@cancer.org?subject=Re:+An+American+Cancer+Society+Perspective

I am writing to indicate that the American Cancer Society (ACS) does not endorse or agree with the recently published study on environmental tobacco smoke (ETS) by Enstrom and Kabat (1), even though some of the data were originally collected by ACS.

Scientifically, the fatal flaw of the paper is that the information collected on environmental tobacco smoke (ETS) exposure is insufficient to distinguish persons who were exposed from those who were not. When the study began in 1959, no information was collected on potential ETS exposure other on the smoking behavior of the spouse. At that time, exposure to second-hand smoke was pervasive in the United States and virtually everyone was exposed to ETS either at work, in social settings, or in other activities of daily living. Thus, the comparison group of "unexposed" persons whose spouses did not smoke was highly exposed to other sources of ETS, both before the study and during at least the first decade of follow-up. After 1972, the potential for misclassification of exposure was perpetuated and magnified, since no further information was collected on smoking by the spouse or on other sources of ETS exposure during the remaining 26 years of follow-up. Many of the spouses who reported smoking at the start of the study would have quit, died, or ended the marriage, yet the surviving partner was still classified as "exposed" in the analysis. The long duration of follow-up is a liability rather than a strength of the study with respect to the resultant misclassification of ETS exposure.

It is extraordinary that Dr. Enstrom persisted in his analysis of ETS exposure in the California subset of Cancer Prevention Study I (CPS-I), despite repeated cautions from me and other ACS epidemiologists that his long-term follow-up of CPS-I would not be informative about this issue. Both he and Dr. Kabat are aware that CPS-I was not designed to study ETS and that the exposure data on ETS in the overall CPS-I cohort (2) and especially in the long-term analyses of the California subgroup are far less informative than the exposure data in many of the studies they criticize (1). Their paper mentions the reportedly "negative" results published by two other tobacco industry consultants on ETS and heart disease in CPS-I and CPS-II(3). It does not mention that the analyses by these consultants of CPS-II was shown to be invalid when replicated by ACS epidemiologists, (4), or that the effects of ETS could not be evaluated in CPS-I because of problems in determining ETS exposure (2). ACS epidemiologists have stopped collaborations with Dr. Enstrom, because of his intransigence on this and other issues. We were unaware, however, that he applied for and received funding from Phillip Morris in 1998 (5) while still negotiating with Dr. Clark Heath to develop a follow-up arrangement of CPS-II participants in states other than California. His failure to inform ACS of this interaction with Phillip Morris was completely inappropriate.

Equally distressing from a scientific and public health perspective is the exaggerated importance that the authors attribute to their findings in their conclusion to the paper and in public statements. As of June 2002, when the International Agency for Research on Cancer (IARC) reviewed on involuntary exposure to tobacco smoke and cancer (6), there were 49 studies of ETS exposure from a smoking spouse in women and nine in men. Based on these and other lines of evidence, IARC concluded that involuntary exposure to tobacco smoke is carcinogenic to humans. It is curious that in the present study, the 95% confidence intervals around the point estimate for lung cancer overlap with the overall RR estimates in the IARC review. In other words, the findings of this study do not even differ significantly with the IARC estimates. It is unfortunate that the study by Enstrom and Kabat will be widely cited by the tobacco industry to delay restrictions on public smoking.

(1) Enstrom J, Kabat G. Environmental tobacco smoke and tobacco related mortality in a prospective study of Californians, 1960-98. BMJ 2003;326:1057-61. (2) Thun MJ, Henley J,

Apicella L. Epidemiologic studies of fatal and nonfatal cardiovascular disease and ETS exposure from spousal smoking. Env Health Perspectives 1999;107 (suppl 6):841-6. (3) LeVois M, Layard M. Publication bias in the environmental tobacco smoke/coronary heart disease epidemiologic literature. Reg Toxicol Pharmacol 1995;21:184-91. (4) Steenland K, Thun MJ, Lally C, Heath C. Environmental tobacco smoke and coronary heart disease in the American Cancer Societ CPS-II cohort. Circulation 1996;94:622-8. (5) http://tobaccodocuments.org/pm/2063654067 (6) IARC. Tobacco Smoke and Involuntary Smoking. Vol 83. http://193.52.164.11/htdocs/monographs/vol83/02-involuntary.html ed. Lyon: International Agency for Research on Cancer; 2002.

Competing interests: None declared

Defining a set of difficult issues 20 May 2003

richard horton,
editor
the lancet

Send response to journal:
Re: Defining a set of difficult issues

Email richard horton:
mailto:richard.horton@lancet.com?subject=Re:+Defining+a+set+of+difficult+issues

Many respondents to this debate have expressed passionate views on both sides of the argument over passive smoking - and over this specific paper. These exchanges have provoked some soul searching here at The Lancet too about how we might have handled such a paper - and, indeed, how we do handle papers that are laden with industry conflicts. Here are some further issues that might deserve additional consideration.

1. The values underpinning medical journals: Much has been made about the science of this tobacco-industry sponsored research. Some correspondents have argued that no work funded by tobacco companies should be published in the BMJ (and presumably other medical journals). Others have said that these are matters of science not ethics. This division bears further thought. Medicine is not a purely scientific discipline. The values of medicine are different from the values of science. Lewis Wolpert argues that science is value-free. I doubt that any doctor would argue that medicine is value-free. So, if values matter in clinical practice they must also matter in medical journals. Should medical journals publish work funded by an industry that makes a product that kills? This is a moral question that goes to the heart of medicine - not a scientific issue.

2. For example: if we received a paper on cluster bombs - reporting that they were safer for civilian populations than had been previously believed - and which had been sponsored by the manufacturer of cluster bombs, would we send it out for peer review? If the answer is no, then what is the difference in principle between the makers of cluster bombs and tobacco manufacturers? If there is no difference, then logic insists that the latest passive smoking paper should not have seen the light of day in a medical journal. If the answer is yes, then what is our responsibility as editors in a profession that hopefully believes in health as a human right, a right that is surely antipathetic to the weapons of war - and so, logic again, tobacco?

3. What of consequences? We faced this with the MMR paper by Andrew Wakefield. Does a medical journal have a responsibility to weigh up the consequences of publishing controversial research? The Wakefield paper led to substantial falls in MMR uptake. The BMJ paper on passive smoking was immediately picked up and highligted on the home page of BAT's website - with a linked statement claiming that the BMJ paper supported their view that a ban on smoking in public places was unjustified.

4. Putting these arguments together, editors make three very separate sets of judgements in their decisions about manuscripts - and these distinctions need to be clarified in this debate. First, decisions concerning the moral basis of the work as set against the values of medicine. Second, decisions about the scientific value of the research. Third, decisions about the likely consequences of publishing that work and how those consequences, if adverse, might be ameliorated (an editorial, press release etc). The decision calculus, as our MMR example showed, is complex and needs to be recognised as such. Passionate condemnation or support seems out of place if we are to improve our decision making.

5. On the subject of tobacco sponsorship of research:

(i) What if the tobacco industry sponsored work - and good work too - that showed that passive smoking was associated with higher levels of coronary disease? Would this be acceptable for consideration by a medical journal? Or should all tobacco sponsored research be banned from medical journals? The debate so far has raged about the sponsorship and the methods/result - but they are two, or maybe three, distict issues.

(ii) What if the tobacco industry sponsored work that had nothing to do with tobacco - eg, about the safety of playgrounds (one example given to me). Would that work be acceptable? Or would it, irrespective of the benefit it could bring to children's safety, be unacceptable because of the nature of the tobacco business? What is it exactly that we are objecting to when we react against tobacco industry sponsored work?

6. Money and medical journals: The Lancet publishes research sponsored by the pharmaceutical industry. Some of these papers sell a great many reprints that bring money into the journal. Thus it could be argued that we are profiting directly from the pharmaceutical industry. It is also probably the case - I am not sure - that my work pension is partly invested in the tobacco/pharmaceutical industries. Does this badly compromise me as a editor? And what should I do about it? Does this present a damaging conflict for me? Similarly, for the BMJ. Has the tobacco industry bought reprints of the latest passive smoking paper for their own promotional purposes? If so, does that mean the BMJ has profited from the tobacco industry? By publishing this paper and creating the stir that it has, does the BMJ profit in any way - impact factor, publicity etc - and is the revenue that flows from this coverage a serious issue for the journal? This is a problem that The Lancet, BMJ, and many other journals face - ie, does the source of the revenue that supports our journals matter in terms of judging the independence and integrity of the publication? If so, perhaps we need to be clearer about what is acceptable and what is not.

Richard Horton

Competing interests: None declared

Science as PR 20 May 2003

Gene Borio,
Webmaster, Tobacco.org
10012

Send response to journal:
Re: Science as PR

Email Gene Borio:
mailto:gborio@mindspring.com?subject=Re:+Science+as+PR

This letter questions the presentation of this study as if the British Medical Journal were an arm of

Burson Marsteller. It examines how this presentation fits into the industry's 15-year crusade against secondhand smoke research. It questions the role of study author James Enstrom in that crusade. And it criticizes the seemingly inadequate due diligence by the BMJ.

I believe ignoring or minimizing these issues represents a significant failure of nerve by the BMJ. Science's inability to safeguard its castle from Trojan Horses leads inevitably to the distrust of all science by the public, and, of course, an absence of scientific trust is a win for the industry.

I'm very troubled by the BMJ's overt politicization of this study.

It's my understanding that reputable scientists and journals are very careful about calling any single study definitive--especially when it contradicts a large body of previous studies. I also understand that a null-result study is rarely newsworthy. In addition, I have seen that scientific professionals often take care to minimize the impact a misunderstood study might have. A study on the health benefits of alcohol, for example, is likely to couch its conclusions in moderate terms, for fear of spawning headlines in the popular press like, "Drink up, me maties!"

But this study not only got the cover, it got a politically inflammatory cover at that. "Passive smoking may not kill" is coupled with a picture of a California building's toxic substances advisory. Many will infer -- as at least one "FORCES" site 1 featuring the cover has -- that a larger point is being made beyond the study itself: an implication that this study is so accurate and so powerful that it may invalidate communities' smoking bans.

But this study did find something: an increase in COPD diseases. Yet the COPD result is downplayed, and the null result trumpeted on the cover. The New York Post itself couldn't have hailed this study with more glee. I wonder how many previous covers the BMJ has done heralding null results.

Such a cover practically invites headlines like:

-- "Study denies link between secondhand smoke, disease" (Baltimore Sun)

-- "2nd Study Confirms 2nd Hand Smoke Harmless" (Rush Limbaugh site)

-- "Passive smoking is innocent, says controversial study" (AFP)

-- "Passive smoke 'not bad'" (The Sun)

-- "Passive smoking may not be harmful, says study" (online.ie)

-- "Secondhand Theory Blows Smoke: Study" (NY Post)

-- "Passive smoking may not damage your health after all, says research" (Daily Telegraph)

-- "Passive smoking risks in doubt, study says" (London Times).

This politicization of a scientific study is especially disturbing when one considers the study's funding history, the BMJ's foolish excuse for paying little attention to such funding, the article's vague and minimalist description of the authors' tobacco work, the number of industry consultants listed in the References, and, as topping to the political cake, Enstrom's crucial role in the decade-long industry campaign to characterize the 1993 US EPA report as junk science.

More specifically:

1. The authors write, "In recent years JEE has received funds originating from the tobacco industry for his tobacco related epidemiological research because it has been impossible for him to obtain equivalent funds from other sources."

a. That is a shame. But isn't this the same excuse for accepting tobacco money given by dance companies, battered women shelters, teen smoking prevention programs, and certain University Corporate Responsibility Centers?

This very rationalization is eminently useful by the tobacco industry in a multitude of venues. It is used by recipient organizations in testimony against community smoking bans. It is used to extol tobacco companies' essential benevolence in society, helping the business end continue quietly and profitably. It is used to exonerate tobacco companies in litigation. An obscene amount of money can take one very far indeed.

b. Doesn't the BMJ get suspicious as to why only the industry would fund this study? The clear pattern of industry funding for 40 years--in fact, its very goal, according to what we read in the secret documents-- appears to be to protect the industry's profits. This study certainly fits right in with that goal. Why does this raise no red flag at all at the BMJ?

c. How long, exactly, is Dr. Enstrom's "recent years." This seems coy indeed. W are not told exactly how many tobacco organizations Mr. Enstrom has worked with, in what capacity, or for how long. Surely, considering the industry's history and stated goals in these matters, this is vital knowledge. CIAR, yes, Philip Morris, yes, for this particular study. What about earlier documents linking Enstrom to RJR, and to its law firm Womble Carlyle, which helped RJR fight the Butler secondhand smoke suit.

What of this proposal, dated 1992 and attributed in the Bliley documents to Dr. Enstrom? It is entitled, "Report Concerning Smoking and Health Issues Prepared by Consultants Engaged by RJR for the Purpose of Assisting Attorneys in Connection with Ongoing Litigation Transmitted to RJR in-House Legal Counsel":

"The mortality changes among nonsmokers must be due to factors other than cigarette smoking and these changes mean that factors other than personal smoking have had a major impact on mortality rates that has not been fully considered when evaluating the impact of cigarette smoking. These changes have a direct bearing on the interpretation of the death rate changes that have occurred among the population as a whole, presumably due to changes in smoking. Also, these changes affect the interpretation of the number of U. S. deaths "due to" cigarette smoking . . . " 1

And what of this 1996 letter, from J. Furr of Womble Carlyle?

"Re: EPA Litigation -- Enstrom Publication

"Dear Dan, Jim and Jim:

"Enclosed please find a copy of the analysis of the Fontham study that Dr. Enstrom has submitted for publication in JAMA I will keep you apprised of the status of Dr. Enstrom's submission.

"I also will be calling to discuss meeting with Dr. Enstrom regarding additional work that he has proposed." 3

c. And what of Dr. Enstrom's role in the fight against the EPA's 1993 report? Enstrom was liberally quoted in several influential if not seminal articles that found fault with the report -- one by Michael

Fumento and two by Jacob Sullum.

In 1994, Sullum's articles were reprinted in full-page ads nationwide by Philip Morris and RJR. RJR's run was headlined, "If we said it, you wouldn't believe it."

If indeed Dr. Enstrom was being funded by the industry at this time, this PR seems patently dishonest -- the industry funds a person it likes (to put the most naive face on it), then touts his opinions proclaiming, "We didn't say it, he (presumably, a disinterested scientist totally unaligned with the industry) did!"

Deployed for political and business goals, such a manufactured and circular citation ill becomes a scientist who would partake in the deception simply by his silence. It is certainly unbefitting a scientist who should be published in--and excused by--the British Medical Journal.

Did the journal know of this history? Did it think it irrelevant? Did it even ask? Unless the BMJ has investigated these issues thoroughly in light of these documents and Enstrom's history, I believe the vague "recent years" admission cum excuse is far too weak a descriptor to constitute a full disclosure of conflicting interests.

(I certainly was comforted, however, by the authors' ultra-scientific disclosure that they are "both lifelong non-smokers" -- almost as comforted as when I read a similar avowal by John Luik in his Bostonia magazine article "Pandora's Box." Objective gentlemen, all!)

2. To what is Prof. Smith referring when he says, "the controversy still persists?" I know of exactly two non-tobacco-funded scientists who dispute some aspects of the secondhand smoke literature. Two scientists hardly make a controversy. To this day, Jacob Sullum continues to cite Enstrom as proof of a "controversy" about secondhand smoke--as if there are simply too few non-tobacco-funded sources available to maintain his claim of a controversy. (It's certainly striking how many tobacco-funded scientists are cited in this study's references.)

The only controversy I have seen in this matter is tobacco-funded, just like this study, which Prof. Smith says will "continue the controversy." It's as if Prof. Smith is in dutiful lockstep with the Philip Morris' 1988 strategy to "keep the controversy alive." 4

3. The BMJ's justification of tobacco industry funding seems to be based on the scientifically-accurate but unuseful adage that even a stopped clock is right twice a day. Even so, I believe this policy should be reworked in the case of the tobacco industry. After all, the industry's outsize PR capability can make a stopped clock's 2 seconds appear more important than a running clock's 86,400. When a funding source has such a nefarious history, and has demonstrably used science for its own political and business goals, I believe the BMJ must formally adopt strict rules governing the examination of that source's submissions, authors and provenance--a due diligence that goes as far beyond the usual as the industry's historic depredations have gone.

4. If the BMJ did know of Enstrom's history, and the history of the tobacco industry's massive and variegated war against the science of a) primary smoking and b) secondhand smoke, then why did the BMJ create -- so boldly, so baldly -- a presentation of this study that any industry PR flack would be proud of? Before trumpeting a study that finds the industry's stopped clock is right more often than the medical world's 2-minutes-fast clock, some editorial caveats would seem to be in order. It's singularly unbecoming for the British Medical Journal to act as an arm of Burson-Marsteller.

If the BMJ can be this lax about safeguarding against manipulation by the industry, what does this say to lesser journals? How big a flood of tobacco industry studies polluting the literature may we look forward to?

This can destroy the integrity of not just the BMJ, but of the public's faith in the scientific establishment. If science is incapable of policing itself--in fact, if it deliberately lends itself to a study funders' political goals--then all trust is lost.

Which, of course, would be just fine for a multitude of commercial interests. If trust in science dies, PR, ignorance and deceit will rule in its place.

The industry certainly got its money's worth on this study.

"Those who cannot remember the past are condemned to repeat it." -- George Santayana

1. "The secondhand smoke myth goes up in smoke," http://www.forcesduluth.com/ Downloaded May 18, 2003

2. "Report Concerning Smoking and Health Issues Prepared by Consultants Engaged by RJR for the Purpose of Assisting Attorneys in Connection with Ongoing Litigation Transmitted to RJR in-House Legal Counsel": http://tobaccodocuments.org/bliley_rjr/515814425-4435 .html, accessed May 16, 2003

3. "Epa Litigation -- Enstrom Publication." 09 May 1996. http://tobaccodocuments.org/rjr/515638685-8685.html, accessed May 17, 2003

4. "Note on a special meeting of the UK Industry on Environmental Tobacco Smoke, London, February 17th, 1988" http://www.ash.org.uk/papers/401247331.pdf, http://tobaccodocuments.org/youth/EtPrPMI19880217.M m.html, accessed May 17, 2003

Competing interests: Tobacco.org, a news-tracking website that posts all tobacco news without censorship, is currently funded by TTAC, which was established in 2001 through a grant from the American Cancer Society, the American Legacy Foundation, and The Robert Wood Johnson Foundation. As on the website, I speak only for myself.

Read peer review comments 20 May 2003

Kamran Abbasi,
Deputy editor
BMJ

Send response to journal:
Re: Read peer review comments

Email Kamran Abbasi:
mailto:kabbasi@bmj.com?subject=Re:+Read+peer+review+comments

Today we posted the peer review comments for this paper.

If you go to the full article and look in the box in the top right hand corner you will find the link. There is also a temporary link from the BMJ's homepage.

This paper underwent an initial round of review and then the paper and the reviewers' comments were discussed at an editorial advisory committee meeting. This committee decided to reject the first submission but offer to consider a revised paper. The revised paper was reviewed again by two of the reviewers before final acceptance.

The BMJ is among the few medical journals that have an open peer review process. This is open in

the sense that the identities of the authors and reviewers are not concealed from each other.

Competing interests: None declared

The Correlation of Smoking and Deep Breathing 21 May 2003

Bernard X. Bovasso,
none
self-employed; 12477,
none

Send response to journal:
Re: The Correlation of Smoking and Deep Breathing

Email Bernard X. Bovasso, et al.:
mailto:bernx@aol.com?subject=Re:+The+Correlation+of+Smoking+and+Deep+Breathing

Regular deep breathing is tonic in effect. Smoking is a form of deep breathing. Ignored in these studies about the more or less toxicity of second hand smoke is how the smoker enjoys a form of deep breathing whereas the breather of second hand smoke does not.

Accordingly, it should be reminded to persons who are able to quit smoking that they must continue some form of regular deep breathing. In the abscence of such practice the former smoker becomes phobic about second hand smoke and all of which has to do with the abscence of the benefits of deep breathing rather than the toxicity of tobacco smoke. And of course those who indulge their anti-smoking phobia appear less alarmed at the constant presence of exhaust fumes from motor vehicles and which is far more toxic than tobacco smoke. In either case, shallow breathing runs the greater risk to health.

Bernard X. Bovasso
bernx@aol.com

Competing interests: None declared

Reviewers' comments 21 May 2003

Martin McKee,
Professor of European Public Health
LSHTM, Keppel St., London WC1E 7HT, UK

Send response to journal:
Re: Reviewers' comments

Email Martin McKee:
mailto:martin.mckee@lshtm.ac.uk?subject=Re:+Reviewers'+comments

Dear editor,

Thank you for publishing the reviews of this paper, not least because they appear to confirm the concerns voiced by many earlier correspondents. Professor Evans, in his second review, notes that "The problems with misclassification of exposure are not dealt with as simply as they state" and that "I would prefer to see that more reservations are included about the strength of conclusions that can be drawn from epidemiology of this type." This is certainly not the message conveyed by the paper or, more importantly, by the BMJ's press release, and, unsurprisingly, by the subsequent media coverage. His comment that "...though there are hints that the authors are less open-

minded than they suggest" seems particularly prescient in view of what is now known.

George Davey Smith's comments about the interpretation of the findings "I do not think these are negative findings – they are based on a small number of cases and therefore imprecise and the confidence intervals for the sex-specific analyses include one, but the best estimate is of a relatively substantial effect" again was not taken up in the final paper and nor was the point that he made in his editorial that combining males and females produced a significant result for COPD.

As an editor who has been misled by an ostensibly independent scientist later found to be a consultant for the tobacco industry, I am hesitant to criticise others who may find themselves in a potentially similar position. Discovering the full story can be a lengthy and painful task [1].

The question, for reviewer and editor, is often not about the scientific merits of what was published but instead about the many analyses that were not submitted for publication. A secondary issue is the difficulty in inferring motives to statements that could be seen as genuine differences of interpretation but may be viewed otherwise in the light of other evidence. In these circumstances it is essential to go beyond the paper in front of you to look at the background to the study, as we have done in our earlier rapid response. It is extremely regrettable that this should have to happen but bitter experience has shown it to be the case in research on passive smoking and disease, a link that the tobacco industry has expended unprecedented resources to seek to undermine. In these circumstances we now need to know whether the manuscripts received by the reviewers included statements of competing interests.

The issue now is what to do next. In recent cases where papers have provoked major controversy the BMJ seems to have adopted a policy of simply counting the numbers of responses for and against. This approach is not acceptable in this case, even though, at least so far, it would show very widespread concern. Earlier rapid responses using industry documents indicate clearly that there many unanswered questions about this paper and the role played by senior tobacco industry executives and their leading consultants in the final analyses, in particular at the meeting in June 2000 [2]. One has also raised questions about the validity of the data for this study, given the concerns apparently voiced by the American Cancer Society.

When we faced questions about competing interests by authors of a paper we had published on passive smoking we undertook a full investigation, producing evidence that was subsequently used successfully in a legal action in Switzerland. I would encourage Richard Smith to initiate such a review now as a matter of urgency.

Martin McKee
Editor in Chief
European Journal of Public Health

1. McKee M. Smoke and mirrors: clearing the air to expose the tactics of the tobacco industry. Eur J Publ Health 2000; 10: 161-163.

2. Sanders T. Tobacco smoke - lung cancer - histological types and analysis of major databases. Bates Number: 2505540920

Competing interests: See previous rapid responses

Apology Requested 21 May 2003

William T Godshall,
Director, Smokefree Pennsylvania

Pittsburgh, PA 15217 USA

Send response to journal:
Re: Apology Requested

Email William T Godshall:
mailto:bill@smokescreen.org?subject=Re:+Apology+Requested

To begin atoning for publishing and promoting "Environmental tobacco smoke and tobacco related mortality in a prospective study of Californians, 1960-98" by James E. Enstrom and Geoffrey C Kabat, BMJ editors are urged to publish at least as bold of apologies on the front cover, in an editorial and in the promotional press release of a forthcoming BMJ issue.

According to the article's competing interests, "In recent years JEE has received funds originating from the tobacco industry for his tobacco related epidemiological research because it has been impossible for him to obtain equivalent fund from other sources." But a quick search of tobacco industry documents reveals a long term mutually beneficial relationship between JEE and the tobacco industry (chronologically listed below).

1975: http://legacy.library.ucsf.edu/tid/ldi79c00 Enstrom writes to the tobacco industry funded and controlled Council for Tobacco Research (CTR) requesting funds.

1975: http://legacy.library.ucsf.edu/tid/qdi79c00 Enstrom writes again to CTR asking for money.

1976: http://legacy.library.ucsf.edu/tid/quk2aa00 Enstrom writes to Tobacco Institute asking for help in getting CTR money.

1976: http://legacy.library.ucsf.edu/tid/guk2aa00 Enstrom writes to CTR about his grant application.

1978: http://legacy.library.ucsf.edu/tid/hgi89c00 Enstrom writes to CTR to ask for money again.

1979: http://legacy.library.ucsf.edu/tid/gov10f00 Enstrom's research finds that lung cancer rates are rising in nonsmokers, leading him to conclude that numerous lung cancers are due to something other than cigarette smoking.

1990: http://legacy.library.ucsf.edu/tid/xoj52d00 Enstrom writes to Philip Morris asking for money "My epidemiological research does not deal directly with the issue of environmental tobacco smoke...However my research does deal extensively with cancer and other diseases among nonsmokers."

1991: http://legacy.library.ucsf.edu/tid/aix89c00 Enstrom applies for more CTR money.

1992: http://legacy.library.ucsf.edu/tid/xix89c00 CTR gives Enstrom a three year grant; goal of research: answer "questions regarding the among of current mortality that can be considered directly due to cigarette smoking and the amount of current mortality that can be prevented by smoking cessation".

1993: http://legacy.library.ucsf.edu/tid/iix89c00 Enstrom gets $34,500 of CTR money.

1994: http://legacy.library.ucsf.edu/tid/mix89c00 Enstrom gets another $34,500 of CTR money.

1995: http://legacy.library.ucsf.edu/tid/gjx89c00 Enstrom gets another $35,000 of CTR money.

1996: http://legacy.library.ucsf.edu/tid/jkx89c00 Enstrom applies for more CTR money.

1997: http://legacy.library.ucsf.edu/tid/elx89c00 CTR offers Enstrom more money.

1997: http://legacy.library.ucsf.edu/tid/lkx89c00 Enstrom gets another $35,000 of CTR money.

1997: http://legacy.library.ucsf.edu/tid/oxw91d00 Enstrom applies to the tobacco industry funded and controlled Center for Indoor Air Research for $425,000 to research passive smoking, which appears to be the funding for his article in last week's BMJ.

1997: http://legacy.library.ucsf.edu/tid/tww91d00 Enstrom's cover letter for the grant application: "For the past three years I have done consulting and research on passive smoking for Jeffrey. L. Furr of Womble Carlyle on behalf of R J Reynolds and Philip Morris." The bait: "This research has found a number of results that raise serous questions about several published findings on the relationship of passive smoking to lung cancer and other diseases".

1997: http://legacy.library.ucsf.edu/tid/imf37d00 Enstrom's grant application to "Philip Morris Research Center".

1997: http://legacy.library.ucsf.edu/tid/cmr29c00 Internal industry review of Enstrom's proposal doesn't consider it important, but notes he "seems to have good connections/resources which might be useful in the future for other issues." This is the same document that has been cited recently by other health groups.

1998: http://legacy.library.ucsf.edu/tid/mjc36d00 Enstrom writes to CTR asking for more money or time (?)

In short, Enstrom's 25 years of requesting money from the tobacco industry aren't consistent with his claim that he's just "received" money and just "in recent years". And during the past decade, Enstrom has received more than half a million dollars from the tobacco industry. In return, it appears he has delivered a study amazingly useful to his sponsor.

The tobacco industry documents cited in this letter were obtained and summarized by Jon Krueger.

Once again, to help regain its previous scientific and public health integrity, the BMJ should publish and promote apologies to its readers and to the media as soon as possible.

Competing interests: None declared

Jumping out of a 3rd storey window "may not kill" either...

21 May 2003

Peter J Flegg,
Consultant Physician
Blackpool Victoria Hospital, UK, FY3 8NR

Send response to journal:
Re: Jumping out of a 3rd storey window "may not kill" either...

Email Peter J Flegg:
mailto:pjf@volcanomail.com?subject=Re:+Jumping+out+of+a+3rd+storey+window+"may+not+kill"+either...

but it is unlikely to be good for one's health.

Competing interests: None declared

Re: Read peer review comments 21 May 2003

Kamran Abbasi,
Deputy editor
BMJ

Send response to journal:
Re: Re: Read peer review comments

Email Kamran Abbasi:
mailto:kabbasi@bmj.com?subject=Re:+Re:+Read+peer+review+comments

Any of you who read George Davey Smith's first report on the day it was posted on bmj.com (20 May) may have been confused that some of it was duplicated and other parts missing. This error has been put right and the correct version is now available.

Competing interests: None declared

Re: Defining a set of difficult issues 21 May 2003

Zubair Kabir,
Research Fellow
CResT Directorate, St. James's Hospital, Dublin 8.

Send response to journal:
Re: Re: Defining a set of difficult issues

Email Zubair Kabir:
mailto:kabirz@tcd.ie?subject=Re:+Re:+Defining+a+set+of+difficult+issues

Dear Richard,

Just a quick reminder that my previous response published on the 19th of May "BMJ, Impact Factor and Irresponsible Journalism: A 'nasty' nexus?" may give an insight into some of your comments.

Regards.

Zubair

Competing interests: None declared

Old habits new diseases? 22 May 2003

Teresa Ramos,
Assistant Professor
Instituto de Tecnologia Biomedica, Av. Prof. Reynaldo dos Santo 27, 2790-136 Carnaxide, PORTUGAL

Send response to journal:
Re: Old habits new diseases?

Email Teresa Ramos:
mailto:investigacao@incor.pt?subject=Re:+Old+habits+new+diseases?

The data presented in this excellent paper only confirms what simple common sense could have already told us. Thus, once upon a time, in Medical Schools, we were told that "small cell carcinomas" of the lung were only found in smokers. More recently this type of carcinoma started to occur, with a relatively high frequency, in non-smokers. Anti-smoke researchers, of course, associated these findings with passive smoke. These types of arguments always startled me. The reason for my surprise has been the following:

Christopher Colombus found America in the year 1500 ad. As we all know, tobacco was used by the Indians and soon thereafter brought to Europe. Let us assume that it took 100 years for the habit of smoking to be established. If my arithmetics are correct then by the year 2000, smoking had been practised for 400 years! Why then "small cell carcinoma" of the lung, in non-smokers, for the past 15-20 years only?

Before concluding I would like to raise another question regarding the smoking ban on flights. It was surprising how fast the various Airlines adhered to the no-smoking campaigns, in spite of all the problems that such policy brought about. The mystery was unravelled when I understood that a smoking ban saved a lot of many, to the Companies, for without smoking it was believed that carbon and Hepa filters were no longer needed or, at least, the frequency with which they had to be replaced was much lowed. My question is then: as the majority of the air that we breathe in an aeroplane is recirculated without appropriate purification, are we not incurring a very big risk of contracting diseases transmitted by airborne micro-organisms such as SARS?

Competing interests: None declared

Re: Science as PR....Bravo, Gene! 22 May 2003

▲ ▼ Errol E. POVAH,
Deckhand
B.C. Ferries

Send response to journal:
Re: Re: Science as PR....Bravo, Gene!

Email Errol E. POVAH:
mailto:grim.reaper@telus.net?subject=Re:+Re:+Science+as+PR....Bravo,+Gene!

Gene Borio's "Science as PR" is certainly among the best of many excellent responses to yet another tobacco industry-created "controver$y." Sadly, I'm not convinced that, if it was in the industry's "be$t intere$t$" to convince us that the earth is flat, the BMJ wouldn't publish that report too!

In my opinion, the highlight of Borio's response was:

"The BMJ's justification of tobacco industry funding seems to be based on the scientifically-accurate but unuseful adage that even a stopped clock is right twice a day....the industry's outsize PR capability can make a stopped clock's 2 sceonds appear more important than a running clock's 86,400."

Speaking of clocks....

The overwhelmingly rich, powerful, corrupt and 'morally' bankrupt tobacco industry clearly has the power to stop -- and even turn back -- 'the clock' of public health.

It's time the industry had something in common with that clock: It must be stopped.

Bravo, Gene!

Errol E. Povah

Competing interests: Public/worker HEALTH or tobacco industry WEALTH. Which do you support?

Re: Inverse effect can be explained 22 May 2003

Adam Jacobs,
Director
Dianthus Medical Limited, London SW19 3TZ

Send response to journal:
Re: Re: Inverse effect can be explained

Email Adam Jacobs:
mailto:ajacobs@dianthus.co.uk?subject=Re:+Re:+Inverse+effect+can+be+explained

I find it hard to accept the hygiene hypothesis as a reason why passive smoking may be protective against heart disease, as suggested by Wiel Maessen. The hygiene hypothesis states that exposure to micro- organisms may protect against the development of allergy [1]. If Maessen's explanation were correct, we would therefore need to accept that firstly, tobacco smoke is an abundant source of micro-organisms, and secondly, heart disease has an allergic basis. Both of those seem unlikely.

Imperfect age adjustment and woefully inadequate classification of passive smoking status seem much more likely explanations to me.

References:

1. von Hertzen LC. The hygiene hypothesis in the development of atopy and asthma — still a matter of controversy? Q J Med 1998;91:767–771

Competing interests: None declared

Why am I dying from lung cancer caused by second-hand smoke? 22 May 2003

Heather S. Crowe,
Volunteer
Physicians for a Smoke-Free Canada, 1226A Wellington Street, Ottawa, Ontario, CANADA, K1A 3A1

Send response to journal:
Re: Why am I dying from lung cancer caused by second-hand smoke?

Email Heather S. Crowe:
mailto:psc@smoke-free.ca?subject=Re:+Why+am+I+dying+from+lung+cancer+caused+by+second-hand+smoke?

The Editor, British Medical Journal,

I was a waitress for forty years, working all the time in smoky restaurants and banquet halls. I frequently worked at two or three jobs, often for sixty hours a week, just to earn enough money to support my family. I am 58 years old. I never smoked, no member of my family smokes and I do not live with a smoker. The only significant exposure tobacco smoke I have ever had was at work.

In 2002, I was diagnosed with inoperable, terminal lung cancer. My oncologist, my surgeon and the Ontario Workplace Safety and Insurance Board (which has awarded me full compensation for disability acquired at work) have all asserted that my lung cancer came from the only risk factor for lung cancer that I have – exposure to second-hand smoke at work.

If second-hand smoke does not cause lung cancer (as the flawed study by Enstrom and Kabat implies), then why am I dying from lung cancer caused by second-hand smoke?

How many more will die because of misinformation paid for by the tobacco industry and irresponsibly published by the British Medical Journal?

I want to be the last person to die from second-hand smoke. I never expected any help in realizing my dream from the tobacco industry. But I would have expected better from the formerly prestigious British Medical Journal.

Heather Crowe

Volunteer, Physicians for a Smoke-Free Canada 1226A Wellington Street Ottawa, Ontario CANADA K1Y 3A1

Competing interests: None declared

Flawed Study on Passive Smoking 22 May 2003

Michael J. Martin,
Assistant Clinical Professor, Department of Epidemiology and Biostatistics
University of California, San Francisco

Send response to journal:
Re: Flawed Study on Passive Smoking

Email Michael J. Martin:
mailto:mmartinmd@earthlink.net?subject=Re:+Flawed+Study+on+Passive+Smoking

If the British Medical Journal received for consideration an article showing that cigarette smoking was not a cause of heart disease, would it publish that article without first doing substantial investigation to verify that the data base used for the study was a valid one from which to draw such a conclusion? Given the large body of evidence showing that cigarette smoking is a cause of heart disease, I expect that the BMJ would evaluate this type of an article very carefully before agreeing to publish it.

The BMJ recently published an article on passive smoking. That article, by Enstrom and Kabat, suggests that exposure to environmental tobacco smoke is not associated with an increased health risk from heart disease, lung cancer or chronic obstructive lung disease. Given the many studies that have shown a positive relationship between environmental tobacco smoke and a variety of adverse health consequences, including and especially heart disease and lung cancer, I was outraged to read the recent BMJ article.

In fact the American Cancer Society cancer prevention study (CPS I) population used by Enstrom and Kabat to draw their conclusions is not a good one for the type of study that was done. The period of exposure for this population was primarily the 1960's. This was a time when a large percentage of the US population smoked and exposure to environmental tobacco smoke was extremely common. It is almost certain that the vast majority of the adults living in US during the 1960's had substantial exposure to environmental tobacco smoke. Given this, it is not surprising

that Enstrom and Kabat found no significant association between exposure to environmental tobacco smoke and heart disease, lung cancer or chronic obstructive lung disease. This potential (and almost certainly actual) flaw with the study design was not given significant attention by either the authors of the article or George Davey Smith, who wrote the accompanying editorial.

Unfortunately, the tobacco industry will get substantial mileage from this flawed article. The industry will no doubt widely publicize the study and, in doing so, confuse the public and legislatures regarding the risks associated with environmental tobacco smoke. This is very unfortunate and could have been avoided if the BMJ had done a better job of reviewing this flawed manuscript.

Sincerely,

Michael J. Martin, MD, MPH
Assistant Clinical Professor
Department of Epidemiology and Biostatistics
University of California, San Francisco

Competing interests: None declared

PUT YOUR LUNGS WHERE YOUR MOUTH IS 22 May 2003

Emanuel Goldman,
Professor of Microbiology & Molecular Genetics
New Jersey Medical School-UMDNJ, Newark, NJ 07101-1709, USA

Send response to journal:
Re: PUT YOUR LUNGS WHERE YOUR MOUTH IS

Email Emanuel Goldman:
mailto:egoldman@umdnj.edu?subject=Re:+PUT+YOUR+LUNGS+WHERE+YOUR+MOUTH+IS

I have a question for the Editors of BMJ: do you allow smoking within your editorial offices? If not, in light of the 'study' you just published exonerating second hand smoke, will you now change your policy and allow smoking within your offices?

If the answer is 'no', i.e., you do not and will not allow smoking within your editorial offices, you had best publish a retraction and apology for the harm you have caused by airing (no pun intended) this 'study' in what used to be an authoritative journal.

If the answer if yes, perhaps some of your readers may wish to avail themselves of the opportunity to invest in life insurance policies on your staff members.

Emanuel Goldman, PhD
Professor of Microbiology & Molecular Genetics
New Jersey Medical School, Newark, NJ 07101-1709

No competing interests

Competing interests: None declared

Re: Why The Double Standard? 22 May 2003

M. L. Herrin,
Citizen Voter
California 91701

Send response to journal:
Re: Re: Why The Double Standard?

Email M. L. Herrin:
mailto:spinner8@usa.com?subject=Re:+Re:+Why+The+Double+Standard?

When ANY special-interest group or groups gain control of ALL reputable media resources including medical journals like JAMA, Lancet and the BMJ, there IS no science. Instead we have become guinea pigs in a vast, expensive, oppressive experiment in behavior control and manipulation. When these groups and individuals are True Believers and in league with our own government, there's no hope of true science.

The findings in this paper are consistent with 80% of all studies done on shs to date, including the WHO/IARC study of 1998 as you mentioned. If these huge, extensive studies cannot be trusted, particularly when they agree, then NONE can.

Perhaps some of those well-known anti-smokers posting here who claim "no competing interests" can explain why they invariably screech about the funding for a study they can't put to their own uses rather than debating the "science" when they themselves have their snouts deep in the tobacco money trough. Is "tobacco money" only suspect when someone ELSE uses it? Or does it miraculously become pure as the driven snow after they've extorted it from smokers?

Competing interests: None declared

"Serious misclassification of exposure" 22 May 2003

Pascal A. Diethelm,
President, OxyGenève
Geneva, Switzerland (CH-1204)Assessement of exposure

Send response to journal:
Re: "Serious misclassification of exposure"

Email Pascal A. Diethelm:
mailto:diethelm@oxygeneve.ch?subject=Re:+"Serious+misclassification+of+exposure"

Dear Editor,

In his rapid response (An American Cancer Society Perspective), Michael Thun says:

"Scientifically, the fatal flaw of the paper is that the information collected on environmental tobacco smoke (ETS) exposure is insufficient to distinguish persons who were exposed from those who were not."

This view was already very clearly articulated at the Toxicology Forum in February 1990:

"Using the presence of a smoking spouse as an indicator of ETS exposure can lead to serious misclassification of exposure. Based on a survey of nearly 38,000 never- and ex-smokers, Friedman and al. [1] reported that the sensitivity and specificity of using the presence of a smoking spouse as a predictor of actual ETS exposure were quite poor. Thirty nine percent of men and 47% of women married to smokers reported zero hours of exposure at home. Conversely, 49% of men

and 41% of women married to non-smokers reported some ETS exposure." [2]

Interestingly, this statement was made by one of the two co-authors, Dr Kabat, who was chairing a session entitled "Epidemiologic studies of the relationship between passive smoking and lung cancer." In his introductory presentation, Dr. Kabat raised a number of important issues surrounding the assessment of exposure status, observing as a preamble that "This is a greater problem than assessment of disease status, and for some investigators it is the key problem."

This is indeed the key problem with Drs. Enstrom and Kabat paper.

Pascal A. Diethelm
President, OxyGenève

[1] (Reference cited in the text) Friedman GD, Petiti DB, Bawol RD. Prevalence and correlates of passive smoking. Am J Publ Health 73: 401- 405, 1983

[2] Kabat, G.C.; American Health Foundation; Gori, G.B.; Lehr, M.; Fleiss, J.; Lee. "Toxicology Forum. 1990 (900000) Annual Winter Meeting. February 19-21, 1990 (900219-900221). Epidemiologic Studies of the Relationship Between Passive Smoking and Lung Cancer.". 21 Feb 1990. Bates: 508478231-508478247. http://tobaccodocuments.org/rjr/508478231-8247.html

Competing interests: President of OxyGenève, an organisation involved in tobacco control, which receives funding for work on tobacco control.

It's not always easy 22 May 2003

James W Austin,
None
Retired

Send response to journal:
Re: It's not always easy

Email James W Austin:
mailto:austinjr@1access.net?subject=Re:+It's+not+always+easy

Give Enstrom and Kabat a break. It's not easy finding ETS to be harmful to heart or lungs. Just ask Simon Chapman ("Did non-smokers REALLY avoid").

In a report for the National Health and Medical Research Council in 1995, after looking at the data, Mr. Chapman faxed his colleagues and said, in part:

"What I am getting at here, is that a reasonable-conclusion will be that the idea that there is ANY lung cancer caused by ETS in Australia will be seen as a huge joke.

Journalists looking at that table (or being directed to it by the industry) will be hard pressed to write anything other than "Official: passive smoking cleared - no lung cancer"[1]

Chapman also noted that heart disease caused by ETS in Australia was almost nonexistent, only 2.2 percent (my calculation) of what it should've been if Well's claims of heart disease from ETS were true.

It's also my understanding that data was deleted by his colleagues before presenting this report to the NHMRC to fit their agenda.[2]

As to housewives in the early years of this study having exposure levels equivalent to the general population, I think it's fair to say that housewives tended to stay at home. The female working population tended to be young and single. Other than occasional trips to the grocery store and the occasional night out, where would other exposures have come from?

1. http://www.worldsmokersday.org/lies/simon_chapman.htm

2. http://193.78.190.200/who/2308.htm

Competing interests: I enjoy smoking

Judging Research 22 May 2003

freda lee nason,
director of facilities
university of massachusetts, dartmouth, massachusetts 02747

Send response to journal:
Re: Judging Research

Email freda lee nason:
mailto:lnason@umassd.edu?subject=Re:+Judging+Research

Scientific research ought not to be judged based upon the consistency of the findings with our preconceived notions of what "ought to be". Science moves forward by uncovering anamolous facts and discrepancies in data that must then be investigated to uncover the next layer of reality.

You are to be congratulated for having the courage to publish research that, while politically incorrect and therefore destined to be excoriated by the anti-smoker lobbyists (many of whom work for anti- smoking organizations and therefore have obvious conflicts of interest even if they refuse to cite them) meets these criteria. Take solace that you are only being bashed verbally -- Galileo paid a greater price for promulgation of his research that challenged the worldview of the catholic majority.

Who said that a career in science would be dull and safe?

Competing interests: None declared

Re: Six Key Issues 23 May 2003

Emma L Dickinson,
Press Officer
BMA House, Tavistock Square, London WC1H 9JP

Send response to journal:
Re: Re: Six Key Issues

Email Emma L Dickinson:
mailto:edickinson@bmj.com?subject=Re:+Re:+Six+Key+Issues

In response to Ronald M. Davis, I'd like to clarify the way in which the BMJ approaches its media activities.

We make every effort to present the facts of a paper/editorial in our press releases. This release

followed the standard format for BMJ press releases, i.e. the overall message of the research was contained in the first paragraph, and the remainder of the release described in simple terms how the authors arrived at their conclusions.

The cautionary points made by Professor Davey Smith in his editorial were also clearly stated in the press release: "He points out difficulties in studies of passive smoking, such as problems with measurement imprecision, misclassification, confounding factors, and low statistical power, that can lead to the risks being distorted."

Every press release is also read and approved by both the lead author of the paper/editorial and a senior member of the BMJ editorial team for suitability and accuracy, before being issued to the media.

Finally, journalists recognise that our press releases can only provide a brief summary of a research paper and that they are not prescriptions for what should or should not be written. As such, we provide journalists with the full text of the paper and author contact details with every release. We also issue our press releases under embargo, prior to publication, giving journalists every opportunity to make up their own minds about what they read, and to gather information from other relevant sources.

This media process is something we have developed over many years. Admittedly, we don't always get it right and, along with other leading medical journals, have occasionally been criticised for instigating erroneous headlines. In light of your comments, we may look to refine this process further.

Competing interests: I am employed by the BMJ

Lost Credibility 23 May 2003

Christopher Lovelidge,
Publisher
Vancouver Canada V4C 6P5,
http://www.you-are-the-target.com/

Send response to journal:
Re: Lost Credibility

Email Christopher Lovelidge, et al.:
mailto:chryancomm@dccnet.com?subject=Re:+Lost+Credibility

In August 2002 our book You Are The Target (Big Tobacco: Lies, Scams - Now The Truth) was reviewed in the BMJ, and recommended as educational resource material, "a mine of ammunition for tobacco control activists". We considered this review and recommendation a great honour, and it was the source of much pride and professional satisfaction.

The BMJ has now provided global publicity for a tobacco-funded "study" surpassing any tobacco p.r. firm's wildest dreams. We are greatly saddened to see your formerly prestigious medical journal's credibility so greatly diminished in this way.

Competing interests: Publisher, You Are The Target (Big Tobacco: Lies, Scams - Now The Truth) by Georgina Lovell

Response to McKee and Diethelm 23 May 2003

Geoffrey C Kabat,
none
New Rochelle, NY USA

Send response to journal:
Re: Response to McKee and Diethelm

Email Geoffrey C Kabat:
mailto:gck1@earthlink.net?subject=Re:+Response+to+McKee+and+Diethelm

Drs. McKee and Diethelm raise specific points, on which one feels compelled to set the record straight, even though it is depressing to have to descend to this level. As I tried to point out in my letter of May 17, the zealous pursuit of compromising information for its own sake, without paying attention to the work itself, inevitably leads to irresponsible allegations and insinuations.

Drs. McKee and Diethelm want to be "reassured" that I was not involved in work at the American Health Foundation funded by the tobacco industry. During the fifteen years I was employed as an epidemiologist at the American Health Foundation I was supported almost exclusively by grants from the National Cancer Institute on which I was an investigator. I received a salary and was not privy to information on other sources of funding.

What I can say is that I developed an interest in passive smoking in 1981 when the Hirayama study appeared, and I published the results of the first American Health Foundation case-control study of passive smoking in 1984 [1]. Dr. McKee implies that Dr. Ernst Wynder's and my views on the passive smoking question were influenced by funding from the tobacco industry. Ernst Wynder's and the American Health Foundation's contribution to our knowledge of the epidemiology and toxicology of tobacco-related diseases is a matter of record. To imply that skepticism about the "weak association" of passive smoking with heart disease and lung cancer is due to influence from the tobacco industry is simply wrong-headed. There is legitimate debate about the effects of passive smoking on heart disease and lung cancer. The evidence is not as uniform or as strong as the activists and scientists with extra-scientific agendas make out. Interestingly, I have never before had this charge made against any of the work I have published on passive smoking or any other topic.

I do not doubt that there is scientific work which has been skewed by an overt or covert pro-industry bias. But when scientists start digging up "evidence" which is used to taint anyone whose results they object to, something has gone seriously amiss.

What is most disturbing in the uproar we have witnessed in the past few days is the lack of any acknowledgement of the biased and demagogic behavior of many of the anti-smoking activists who have attacked our paper. But then again, as pointed out by others, a worthy goal seems to justify any means.

1. Kabat GC, Wynder EL. Lung cancer in nonsmokers. Cancer 1984; 53:1214-1221.

Geoffrey Kabat, Ph.D., M.Sc. New Rochelle, NY, USA

Competing interests: As stated at the end of the paper by Enstrom and Kabat

The 'overwhelming evidence' for the links between ETS and lung cancer and heart disease

23 May 2003

Wiel M Maessen,
Board member of Forces International

Netherlands

Send response to journal:
Re: The 'overwhelming evidence' for the links between ETS and lung cancer and heart disease

Email Wiel M Maessen:
mailto:wiel@forces-nl.org?
subject=Re:+The+'overwhelming+evidence'+for+the+links+between+ETS+and+lung+cancer+and+heart+disease

How many of the studies reported were funded by the stakeholders from the pharmaceutical industry? When the important medical journals decided to use more strict rules for publication, they forgot to go back in time and remove all the studies that were published about ETS and that were not conforming to the new rules. I wonder how much of the 'overwhelming evidence' will stay put.

The research we have done on the amounts of money spent to anti- tobacco research by only one organisation from the Industry (Robert Wood Johnson Foundation, see: http://www.forces-nl.org/images/rwjf2003.gif) shows that, since 1992, $218,368,343 has been spent on anti-smoking research and support for anti-smoking groups. The figures were extracted from RWJF's own web site's database.

It's also typical that, once the Master Settlement Agreement took care of the anti-tobacco financing, RWJF's funding dropped significantly. At the moment they are reserving funds for the War on Alcohol.

And this is just one organisation that we analysed. I am sure that when figures of other stakeholders were available to the public the amount spent on anti-smoking will be quite a bit more.

I challenge the medical journals, who might be able to trace the funding sources of the studies, to clean up their files from these studies and report on their findings.

Competing interests: None declared

Re: Why am I dying from lung cancer caused by second-hand smoke?

23 May 2003

Wiel Maessen,
Board member of Forces International
Netherlands

Send response to journal:
Re: Re: Why am I dying from lung cancer caused by second-hand smoke?

Email Wiel Maessen:
mailto:wiel@forces-nl.org?subject=Re:+Re:+Why+am+I+dying+from+lung+cancer+caused+by+second-hand+smoke?

Can you proof that statement?

There are lots of other causes for lung cancer: genetical background (RR=2.4 to 5.3), cooking methods (RR=1.4 to 8.3), Radon exposure (RR=2.4 to 4.3), Asbestos exposure (RR=2.3 to 10) and even beer drinking has been reported with an increased risk of 100%[1]. Also psycho-social traits can raise the risk for lung cancer with 100 to 200%[2]. All show significantly higher relative risks than the max RR=1.3 of ETS. And this is just a subset of all the non-smoking related causes reported in studies.

The American Cancer Society and the other professional anti contingents of the so-called "public health community" don't like it to be known that anywhere from 20,000 to 30,000 never-smokers get lung cancer every year. Some of these are very young, like pilot Lisa Wagner, a never- smoker who died from lung cancer in June, 2002. She was 31. Or bagpipe champion Kevin Quail, also 31. Or

Susan Manley, 38, of Cincinnati, who never smoked, never worked around tobacco smoke, and grew up in a non-smoking home. Or Kim Perrot, the champion Women's National Basketball Assn. point guard of the Houston Comets, who was only 32 when she died of lung cancer.

Even vehement anti-smokers get lung cancer. Just this past September, celebrity hairdresser Murdo Maclean died of lung cancer at 52. He not only never smoked (or drank) himself, but he banned smoking in his shop in the 1970s and was vehement about his opposition to tobacco smoke and smoking.

And, despite all the smoking bans, the incidence of lung cancer in never-smokers appears to be rising, even in men. According to a study by IARC (an arm of the World Health Organization), the rate of lung cancer in a cohort of never-smoking Swedish men was more than four times as high between 1991 and 1995 as it had been between 1976 and 1980[3].

Further, about 50% of diagnosed lung cancer cases are in EX-smokers, some of whom have not smoked in 30 or more years, like Bernard Fox, retired chief executive officer of Northeast Utilities in Berlin, Connecticut: "I had not smoked in more than 30 years, and I had kept in excellent physical condition. In a nutshell, lung cancer was the last disease I thought I would get," [Bernard M. Fox, "Uphill Battle," Hartford Courant, 12/4/01].

The specialists you mentioned will never have been able to check all possible causes for the horrible disease you're suffering from.

But it definitely served their political agenda.

---

[1] Potter J, et al. , Alcohol, Beer and Lung Cancer in Postmenopausal Women: The Iowa Women Health Study. Ann. Epidemiol., 2:587-95 (1992).

[2] Kulessa C, et al.: Psychosocial Personality Traits and Cigarette Smoking Among Bronchial Carcinoma Patients. Stress Med., 5:37-46 (1989). Knekt P, et al.: Elevated Lung Cancer Risk Among Persons With Depressed Mood. Am. J. Epidemiol., 144:1096-103 (1996).

[3] Bofetta P, Jarvholm B, Brennan P, Nyren O, "Incidence of lung cancer in a large cohort of non-smoking men from Sweden," International Journal of Cancer, 94:4, pp. 591-593, 8/27/01

Competing interests: None declared

Re: Judging Research 23 May 2003

Nigel R Winterbottom,
Manager, Information Technology
Mississauga Ontario Canada L5A 4A1

Send response to journal:
Re: Re: Judging Research

Email Nigel R Winterbottom:
mailto:nigelwinterbottom@yahoo.ca?subject=Re:+Re:+Judging+Research

I enjoy eating fat-laden food, hate fruit and vegetables with a passion and I do not yet smoke (although might decide to one day).

I am not yet obese (In spite of aspiring to this goal !!) and I am willing to undertake minor risks in order to extract pleasure from my miserable existence on this planet..... Such pleasures do not involve working out at the gym regularly or jogging on roads facing oncoming traffic.

If I die prematurely (After age 65) as a result of politically incorrect lifestyle choices, then I can rejoice in the societal cost savings in pensions and health care expenses that have accrued from my shortened life span.

Many of us as taxpaying citizens are tired of hearing about what is and isn't good for our health, especially when assertions appear to be so routinely based on distorted science and exaggerated risks. We are definitely not impressed when legislation is implemented to ensure our compliance with behaviours approved by health fanatics, particularly when these manoeuvres are largely funded by a combination of either corporate Pharmaceutical interests or our own taxes. I dread to contemplate the number of people currently employed within tax funded "anti-something" industries eating up our hard earned money like there is no tomorrow.

Moreover, the politicisation of health issues by organisations such as the W.H.O, National/ Local government Health Officials and various Non- Governmental organisations makes me cringe. Health institutions used to be reasonably well trusted but are now commonly thought of as lacking integrity. As Mr Gooch and Freda Lee Nason expressed in an earlier posting, personal prejudices are all too frequently allowed to obscure the simple truth or trample legitimate doubt in a frenzy of political correctness.

It appears to me that dissenting opinions are rarely allowed to surface without risk of expulsion from the establishment. In this regard I certainly applaud the BMJ for publication of this study, it takes a lot of courage to publish something carrying a result which is unfavourable to the prevailing political environment.

Honesty and integrity are essential if the Health establishment is to retain any shred of credibility. The publishing of this study is a step in the right direction.

I am an ordinary citizen NOT employed by the tobacco or food industries and I am certainly not employed by any of the following organisations who I resent funding with my taxes :-

Non Smokers Rights Organisation
Physicians For A Smoke Free Canada
Action On Smoking And Health

Competing interests: None declared

Re: Re: Inverse effect can be explained 23 May 2003

Wiel M Maessen,
Vice-president Forces International
Netherlands

Send response to journal:
Re: Re: Re: Inverse effect can be explained

Email Wiel M Maessen:
mailto:wiel@forces-nl.org?subject=Re:+Re:+Re:+Inverse+effect+can+be+explained

This is a very narrow interpretation of the hygiene hypothesis. What works for bacteria may equally work for viruses and.... tobacco smoke.

The effect of tobacco smoke on the hardening of the immune system could be a good topic for a study. After all, there is an indication that it could work that way. [1]

[1] Does tobacco smoke prevent atopic disorders? A study of two generations of Swedish residents., Hjern A, Hedberg A, Haglund B, Rosen M., Clin Exp Allergy 2001 Jun;31(6):908-14

Competing interests: None declared

What are the tobacco manufacturers saying? 23 May 2003

Andrew S Furber,
Specialist Registrar in Public Health
Eastern Wakefield PCT, Castleford, WF10 5LT

Send response to journal:
Re: What are the tobacco manufacturers saying?

Email Andrew S Furber:
mailto:andrew.furber@ewpct.nhs.uk?subject=Re:+What+are+the+tobacco+manufacturers+saying?

In the aftermath of the publication of Enstrom and Kabat's paper [1] and the review of web based information on passive smoking by Gonzalez [2], it is interesting to see what some of the tobacco manufacturers' websites have to say on the topic. Whilst many make no comment on the subject, there are some who have a lot to say.

British American Tobacco's site [3] tries to dismiss the evidence around passive smoking and health, but nevertheless concludes that "it makes sense not to smoke around infants and young children" and that "smokers should be considerate towards people who suffer from respiratory problems such as asthma." The site also refers to Enstrom and Kabat's paper, the accompanying BMJ press release and the editorial by Davey Smith, but needless to say uses the information from these sources in a selective manner.

Philip Morris International's site [4] does not refer to the BMJ papers and generally gives a more balanced view, but also draws similar conclusions saying that "particular care should be exercised where children are concerned, and adults should avoid smoking around them." In addition, "Philip Morris International believes that the conclusions of public health officials concerning environmental tobacco smoke are sufficient to warrant measures that regulate smoking in public places." Brown & Williamson "recognizes that smoking can be annoying and irritating to non-smokers." [5].

With tobacco manufacturers reaching these conclusions and public opinion increasingly in favour of tighter control of second hand smoke, it is time for governments to act.

References

1. Enstrom JE, Kabat GC. Environmental tobacco smoke and tobacco related mortality in a prospective study of Californians, 1960-98. BMJ 2003;326:1057-60.

2. Gonzalez A. Passive smoking [Website of the week]. BMJ 2003;326:1094.

3. http://www.bat.com/oneweb/sites/uk__3mnfen.nsf/vwPagesWebLive/3ED57411BE0B727880256BF opendocument Accessed 23/05/03.

4. http://www.philipmorrisinternational.com/pages/eng/smoking/Secondhand_smoke.asp Accessed 23/05/03.

5. http://www.brownandwilliamson.com/Index_sub2.cfm?ID=16 Accessed 23/05/03.

Competing interests: None declared

Response to James Austin 23 May 2003

Simon Chapman,
Professor of public health
University of Sydney 2006

Send response to journal:
Re: Response to James Austin

Email Simon Chapman:
mailto:simonchapman@health.usyd.edu.au?subject=Re:+Response+to+James+Austin

James Austin's rapid response defames me and all the members of the 1995 Australian National Health & Medical Research Council's working party on passive smoking in his claim that we "deleted" data "to fit our agenda".

Austin bases his claims on a fax I sent to other members of the group in 1995 [1] where I raised two concerns about an early draft chapter of a report we were writing on the health effects of passive smoking in Australia. The draft contained a table that showed estimated deaths in Australia from passive smoking exposure calculated for age and sex bands. The numbers shown were often expressed as fractions of deaths. My fax argued that "fractional" annual deaths (ie: death rates of less than one per age band) would prove difficult for journalists and the public to understand. For example, I would be surprised if many non-epidemiologists would be able to decipher what "0.5" deaths per annum meant (one death every two years).

There are many perfectly correct ways of expressing the same data in more comprehensible forms, and my fax urged nothing more than that we should realise that the table would cause unnecessary confusion. I subsequently argued in the committee that we should recast the data in a more understandable way. Austin's claim that this means we then "deleted" the data is grossly offensive and wrong, as the final report revealed. It is standard procedure for all draft papers to undergo changes and editing. Often these are to improve clarity of understanding.

Second, I pointed out that our very conservative methodology estimated there to be some 93 annual deaths from ischaemic heart disease caused by passive smoking in Australia, whereas a recent American estimate had put the corresponding US figure at 62,000. Since then the US Environmental Protection Agency has published an 8 volume report estimating some 65,000 deaths. I advised the committee -- correctly – that our report would be therefore "out of step with every international review's conclusion on this subject". In fact, the final report included the same very conservative estimates which resulted from our only considering domestic (spousal) exposure data in people who have never smoked. We did not factor in workplace exposures, nor deaths among ex- smokers.

My fax rehearsed the sort of questions that we were likely to get from those who were familiar with the much higher US mortality estimates. If we had really wanted to massage results to suit "our agenda" why then would we have persisted in using our ultra-conservative methodology which was guaranteed to produce low estimates of deaths?

1. http://www.worldsmokersday.org/lies/simon_chapman.htm

Competing interests: I am editor of Tobacco Control.

Smoke: Cancer ingredients or not 24 May 2003

George F Sedlacek,
Director of Community Health
Marquette County Health Dept

Send response to journal:
Re: Smoke: Cancer ingredients or not

Email George F Sedlacek:
mailto:gsedlacek@hlhe.org?subject=Re:+Smoke:+Cancer+ingredients+or+not

Interesting study and responses. This paper is one of a very few that state that tobacco smoke doesn't seem to do much harm. The article reads like the comic section to me. That anyone would waste time and money anymore trying to convince folks that the carcinogens and CVD accelerators in tobacco smoke are somehow different to those exposed long term is farcical. It seems that the counter point gets scant news headlines, i.e., tobacco smoke is a CVD accelerator, etc., in spite of the fact that dozens of studies a month keep telling the harm caused by tobacco. Ho hum... How many papers would sell with the headline, secondhand smoke clogs arteries faster. Would you or I even respond to the BMJ telling them what a great job they do publicizing health?

The role of the news media is to sell papers and keep controversy alive. Is the BMJ not in the media business? I would bet that most of the folks who put in a comment or two here don't even subscribe. It seems to me that the folks who should care the most, the subscribers could take the market approach and vote with their conscience...maybe then it wouldn't take too long for the BMJ to figure out if it wants to get into the entertainment business.

Someone once said, the best preventive approach to tobacco is to laugh the pushers out of town. It's humerous that the BMJ still feels that tobacco smoke is controversial in its ability to kill folks. I suppose if you felt strongly enough, you could laugh them right off your subscription list.

Competing interests: Pro Health Advocate

The Confounding Urban Factor 24 May 2003

David W. Kuneman,
self
retired

Send response to journal:
Re: The Confounding Urban Factor

Email David W. Kuneman:
mailto:sharz28hus@aol.com?subject=Re:+The+Confounding+Urban+Factor

About the time CPS-1 began, Hammond, et.al., J. Am. Med Assoc., 1958 reported higher smoking rates among urban than rural dwellers. He also found higher lung cancer rates among urban nonsmokers, and higher lung cancer rates in cities even after adjusting for urban/rural differences in smoking rates. According to "Geographic Risks of dying and Associated factors", U.S. Dept of Health and Human Services, series 3, #18, p25, females also have the highest risk of lung cancer in urban areas.

Recent knowlege about urban pollution could easily explain the excess lung cancer reported in some secondhand smoke studies, particularly since the odds a nonsmoker living with a smoker is also an urban dweller are in direct proportion to higher smoking rates in urban areas.

Huper, et.al. Arch. Pathol. 1962 found the fraction of air from 8 U.S. cities induced tumors in experimental animals. Epstein, et.al. Cancer Res. 1972 tested air from New York City and found significant tumorigenic activity in mice. Huber, in Sem. in Resp. Med., 1989 reported animal inhalation studies of environmental toxins, not related to tobacco smoke, do result in lung cancer similiar to that found in man. Finally, Niles, et.al. Analytica Chimica Acta, 1989 found nitrated PAH's in urban air which they claim are some of the most potent mutagens ever identified. The likelihood a nonsmoker living with a smoker is exposed to this urban air is higher, because smokers and therefore thier spouses are more likely to live in cities.

Insofar as heart disease is concerned, urban dwellers are more likely to have occupations with less physical excercise. As a consequence of also being more likely to be living with a smoker, this would lead to the false conclusion they are at increased risk of heart disease because they live with a smoker.

Considering the relatively small odds-ratios most secondhand smoke studies find, it is impossible to obtain an accurate measure of the risk of smoke exposure until the urban factor is controlled properly. As of yet, I have not read any study that attempts to separate the urban factor from the smoke factor.

Competing interests: None declared

Misleading the public about secondhand smoke ... Again 24 May 2003

▲▼▲ Lisa A Bero,
Professor
University of California, San Francisco3333 California St, Suite 420, San Francisco, CA 94118 USA,
Michael Cummings, Stanton Glantz

Send response to journal:
Re: Misleading the public about secondhand smoke ... Again

Email Lisa A Bero, et al.:
mailto:bero@medicine.ucsf.edu?subject=Re:+Misleading+the+public+about+secondhand+smoke+...+Again

Enstrom and Kabat's study is the latest in a long string of studies supported by the tobacco industry to deny the evidence about secondhand smoke and confuse the public. The study has already been widely cited in the lay press and is being used by the tobacco industry to block public health efforts to enact smoke-free policies. The tobacco industry has used their earlier sponsored studies on secondhand smoke in the same ways (1, 2). Enstrom and Kabat's study, the accompanying editorial by Davey Smith, and press coverage of the study fail to acknowledge the validity of reviews of the evidence on adverse health effects of secondhand smoke (e.g., 3). The paper and editorial also ignore the large experimental and clinical literature that defines the mechanisms by which secondhand smoke causes disease, particularly heart disease.

Despite extensive commentary from public health scientists regarding the serious flaws inherent in the Enstrom and Kabat study, some in the public are not hearing the message. The CPS1 data set used for the study was never designed to permit answering the question of whether secondhand smoke causes any disease whatsoever. Using marriage to a smoker in 1959 as a measure for exposure to secondhand smoke over a 40 year period is invalid because virtually everyone during the follow-up period was exposed to secondhand smoke whether they were married to a smoker or not. The lack of an unexposed control group assured a negative conclusion in this study and does not permit the authors to gauge the true effect of secondhand smoke exposure on cancer or heart disease risk.

Thus, the conclusions of Enstrom and Kabat's paper are unwarranted. In addition, it is impossible to know what other, less obvious, flaws might have been introduced by tobacco industry sponsorship of the study. The Enstrom and Kabat study may be another example of the financial disclosure not fully describing the extent of involvement of the tobacco industry in the design, conduct and dissemination of the study (4). For example, most of the studies on secondhand smoke that were funded by CIAR were selected and controlled by industry executives and lawyers, rather than scientists (5). As part of the 1998 Master Settlement Agreement in the United States, CIAR was closed under allegations of fraud.

By publishing Enstrom and Kabat's paper, the BMJ has helped the tobacco industry mislead the public about the harmful effects of secondhand smoke exposure. Only a retraction could stem some of the damages to public health goals that have already been inflicted by this paper.

1. Kennedy G., & Bero L. Print media coverage of research on passive smoking. Tobacco Control, 1999; 8: 254-260.

2. Montini T., Mangurian C., & Bero L. Assessing the evidence submitted in the development of a workplace smoking regulation: The case of Maryland. Public Health Reports, 2002; 117: 291-298.

3. United States Environmental Protection Agency, Respiratory health effects of passive smoking: Lung cancer and other disorders, Document No. EPA/600/6-90/006F, Washington D.C. (1992).

4. Hong M, & Bero L. How the tobacco industry responded to an influential study of the health effects of secondhand smoke. British Medical Journal 2002, 325: 1413-1416.

5. Barnes D., & Bero L. Industry-funded research and conflict of interest: An analysis of research sponsored by the tobacco industry through the Center for Indoor Air Research. Journal of Health Politics, Policy and Law, 1996; 21 (3): 515-542.

Competing interests:

Lisa Bero
Since 1991, Dr. Bero has been a full-time faculty employee of the University of California, San Francisco. Her salary is provided by funds from the state of California and her research grants. She currently receives research grant funding from the National Institutes of Health, California Tobacco-Related Disease Research Program (research money derived from the tax on cigarettes), and Flight Attendants Medical Research Institute. All of these grants have been peer-reviewed by scientific committees. She has also submitted grants to these organizations that have not been funded. In the past, she has received research funding from the American Cancer Society, Robert Wood Johnson Foundation, Agency for Healthcare Research and Quality (AHRQ), and World Health Organization. She has received honoraria (< $500 U.S. per year each) from the Canadian National Cancer Institute and AHRQ (for grant review) and University of British Columbia (for dissertation review). She has received consulting fees ($5000) from the University of Colorado (for a workshop on evidence-based medicine).

Michael Cummings
Dr. Cummings is currently employed as a Senior Research Scientist and is Chairman of the Department of Health Behavior in the Division of Cancer Prevention and Population Sciences at the Roswell Park Cancer Institute in Buffalo, New York, USA. His salary support comes primarily from Roswell Park Cancer Institute and from research funding provided by the National Cancer Institute, the American Cancer Society, the Robert Wood Johnson Foundation, the American Legacy Foundation, and New York State Department of Health. Dr. Cummings serves on the medical advisory board for the Flight Attendant Medical Research Institute (FAMRI) and has served on various scientific advisory boards and grant review committees for National Institutes of Health, Centres for Disease Control and Prevention, American Cancer Society, Canadian National Cancer Institute, Robert Wood Johnson Foundation, and state and local health agencies for which he has received honoraria. Dr. Cummings has also served as a paid expert witness on behalf of plaintiffs counsel in several of cases against the cigarette industry. Dr. Cummings has also received honoraria and has accepted hospitality and on a few occasions, travel costs, from pharmaceutical companies making tobacco dependence treatment products.

Stanton Glantz
Dr. Glantz's research has been funded by the NAtional Institutes of Health, American Cancer Society, American Heart Assoication, and Riobert Wood Johnson Foundation. He occasionally receives honoraria for speaking at meetings sponsored by health agencies.

Response to Simon Chapman 25 May 2003

▲▼ James W Austin,
None
Retired 54703

Send response to journal:
Re: Response to Simon Chapman

Email James W Austin:
mailto:austinjr@1access.net?subject=Re:+Response+to+Simon+Chapman

Mr. Chapman claims I defamed him based upon his fax. If there was any defamation it came from the article I cited, not the fax. But let's first go to the beginning of the fax:

I am DEEPLY concerned about the implications for the credibility of our whole report arising from the calculations found in Tables 7 (p174) and 11 (p179). These tables show that based on an RR of 1.26 there are 9.63 cases of lung cancer and 93.46 from IHD- a total of 103 deaths in a year.

If we look at Table 7 in the way any journalist would, in only two age groups (females 60-4 and 65-9). do the estimates go over 1 death. The lung cancer death aggregate therefore mostly consists of adding up fractions of one death in different age groups. What I am getting at here, is that a reasonable conclusion will be that the idea that there is ANY lung cancer caused by ETS in Australia will be seen as a huge joke. Journalists looking at that table (or being directed to it by the industry) will be hard pressed to write anything other than "Official: passive smoking cleared-no no lung cancer"

I agree with Chapman. Having to add up fractions from different age groups to produce whole deaths does not sound very credible. It's my understanding that epidemiologic studies are crude and inexact.

As Chapman wrote, "is that a reasonable conclusion will be that the idea that there is ANY lung cancer caused by ETS in Australia will be seen as a huge joke."

Well, I believe that is reasonable conclusion and that's without mentioning the fact that not all relevant materials were even taken into account:

In Judge Finn's decision on this report, he said, "I have concluded that the NH&MRC has failed in discharging its statutory duty of public consultation; that it did not give genuine consideration to relevant material in the submissions made to it; and that it denied the applicants procedural fairness." (1)

I can only imagine the "credibility" of this report if they had followed procedure.

As to the deleting of data, and I would guess, the "defamation," Table 7 was deleted as part of an agenda, according to the April 20, 1997 article: Report: Research massaged to push anti-smoking recommendations. It reads, in part:

SYDNEY, Australia (AP) - Australia's principal medical advisory body massaged research results to suit recommendations to ban smoking in public places, an Australian news agency reported Sunday.

The NHMRC report was, however, edited to delete findings inconsistent with the recommendations, the AAP said. In any case, the report did not become public as it was suppressed by a federal court in January this year. The court said the NHMRC had failed to discharge its statutory duty of public consultation and failed to give genuine consideration to submissions in preparing the report.

Chapman subsequently told the AAP that the changes were made to simplify complex medical statistics for the general public.

He said the contentious table was dropped from the report at an early stage because there were other statistics available that illustrated more strongly the dangers of passive smoking.

As I said earlier, the deleting of data I spoke of did not come from the fax, but from this article. I think anybody can see why I said, "further their agenda." Why the switch from one set of statistics to another that, "illustrated more strongly the dangers of passive smoking" if not because of an agenda?

Do you, Mr. Chapman, really believe, "For example, I would be surprised if many non-epidemiologists would be able to decipher what "0.5" deaths per annum meant (one death every two years)."

I'm a non-epidemiologist. 0.5 deaths per annum means one-half deaths per year, not one death every two years as you suggest. And I'm sure other non-epidemiologists can easily make the leap that it would take two years to see one death. I didn't need any help figuring this out.

It's also my understanding that you were head of an anti-smoking group at that time. Do you really expect me to believe that you had no agenda or were impartial?

In "Agent of change: more than "a nuisance to the tobacco industry" you claim that you were once part of a group that defaced tobacco billboards. You even appear proud of this. But unless you were just a common vandal, this points to an extreme agenda and an "end justifies the means" attitude.

But I'm open to compromise. How about in the future I say that data was NOT deleted to further their agenda, but that data was EXCHANGED to further their agenda. Is that okay with you?

You also said, "There are many perfectly correct ways of expressing the same data in more comprehensible forms, and my fax urged nothing more than that we should realize that the table would cause unnecessary confusion."

I never accused you of promoting the deletion/exchanging of data. But the article states the "contentious table was dropped" and that you said other data was used. The data was not expressed differently, it was different data.

Your fax was more than just a warning that "the table would cause unnecessary confusion." You wrote of "looking down the barrell of a MAJOR public relations problem." You "STRONGLY" recommended a face-to-face meeting to discuss what to do about it. You spoke of a "huge banana skin" and urged a top-level strategic discussion on how to handle this. This sounds more like hysteria.

Finally, you asked the question:

"My fax rehearsed the sort of questions that we were likely to get from those who were familiar with the much higher US mortality estimates. If we had really wanted to massage results to suit "our agenda" why then would we have persisted in using our ultra-conservative methodology which was guaranteed to produce low estimates of deaths?"

You might have thought the numbers would be larger; that you would find more than mostly fractional deaths. Why else would a thesaurus be needed if one were using an ultra-conservative methodology and already expecting conservative results? The results were a real shock to you I believe. That's why you have such a reasonable attitude towards the Enstrom/Kabat findings. That's my guess anyway.

Competing interests: I enjoy smoking

Re: Misleading the public about secondhand smoke ... Again

25 May 2003

B.J. Allen,
Teacher
McAllen, TX 78501

Send response to journal:
Re: Re: Misleading the public about secondhand smoke ... Again

Email B.J. Allen:
mailto:ballen@teacher.com?subject=Re:+Re:+Misleading+the+public+about+secondhand+smoke+...+Again

I'd like to know from one of the experts here (preferably one who chose to list his or her "competing interests" since all others are immediately suspect) how many of the hundreds/thousands of studies on secondhand smoke have NOT been funded by either Big Tobacco OR Big Anti- Tobacco? Any of them?

I'm one of the public Mr. Glantz, in his elitist arrogance, fears has been misled by the publication of this study in the esteemed BMJ I beg to differ. I'm quite capable of reading and understanding what this study means, as well as others he'd like to sweep under the rug of Big Tobacco funding.

How long will we, the public, put up with this kind of manipulation and social engineering?

Competing interests: None declared

Wrong conclusion 25 May 2003

William Carey,
Clinical Pharmacologist
Hammersmith Medicines Research, Central Middlesex Hospital, London NW10 7 NS, UK

Send response to journal:
Re: Wrong conclusion

Email William Carey:
mailto:william@carey.demon.co.uk?subject=Re:+Wrong+conclusion

Editor, The results of this study (which are not disputed) lead to the conclusion that any reduction in ETS by being married to a non-smoker is not sufficient to reduce the risk of ill-health caused by passive smoking.

It offers no good evidence as to whether passive smoking does or does not cause ill-health.

Yours faithfully

William D H Carey

Competing interests: None declared

Do any of the "non-smokers" smoke? 26 May 2003

John H. Glaser,
Independent researcher
4 Woodpark Circle, Lexington, MA 02421, USA

Send response to journal:
Re: Do any of the "non-smokers" smoke?

Email John H. Glaser:
mailto:glaserj@alum.mit.edu?subject=Re:+Do+any+of+the+"non-smokers"+smoke?

It is well known that smoking is inversely correlated with education level; the highest percentage of smokers is found among those people who have not completed high school. This clearly casts suspicion on the data entry and/or the computer programming used by Enstrom and Kabat to perform their analysis, because in their report they find that the highest frequency of smoking is associated with the HIGHEST level of education.

From their Table 2 (male never smokers) and Table 3 (female never smokers) sorted by smoking status of spouse, they show that the heaviest smokers (≥ 40 cigarettes/day) are more likely to have completed high school than are non-smokers:

	Education ≥ 12 years	
	Never smoked	≥ 40/day
Table 2	67.3%	84.5%

Table 3	73.7%	77.2%

Further, within the group of smokers, they show that for those smoking a higher number of cigarettes, there is a greater likelihood of completing high school:

	Education ≥ 12 years		
	1-19/day	20-39/day	≥ 40/day
Table 2	71.3%	74.2%	84.5%
Table 3	65.6%	70.4%	77.2%

Clearly, this suggests that there is a flaw in either the data entry or in the programming used to sort the data by smoking status of spouse. The "never smoked/formerly smoked" group, because it does not show the expected high proportion of high school graduates, could contain a significant number of current smokers; that would explain the associated higher rate of heart disease:

	Smoking status	
	Never + Former	Current
Calculated from Table 2: Education ≥ 12 years	68.3%	72.9%
Calculated from Table 2: Heart disease	6.9%	5.6%
Calculated from Table 3: Education ≥ 12 years	71.2%	69.4%
Calculated from Table 3: Heart disease	5.1%	4.0%

Competing interests: None declared

Research Bias and Science 26 May 2003

Michael J. McFadden,
Private Citizen
Home, Pihladelphia PA, 19104

Send response to journal:
Re: Research Bias and Science

Email Michael J. McFadden:
mailto:Cantiloper@aol.com?subject=Re:+Research+Bias+and+Science

I have been watching the debate over Enstrom/Kabat with great interest. It is astounding how much of the criticism springs from an Ad Hominem argument rather than from scientific criticism of the study itself.

The hostility displayed here shows why researchers feel pressured to design and carry out only smoking-related research likely to give results supporting Antismoking efforts. The funding for such research is probably many times greater than any currently coming from Big Tobacco, and it leaves the researchers free from worries that their integrity will be impugned while also promoting their chances for future grants.

I am sure Stanton Glantz was not alone in 1992 (Revolt Against Tobacco, p. 14) when he said, "... that's the question that I have applied to my research relating to tobacco. If this comes out the way I think, will it make a difference? And if the answer is yes, then we do it, and if the answer is I don't know then we don't bother. Okay? And that's the criteria." (1)

All too many scientists today seem willing to subordinate their science to their activist beliefs or the lure of grants from deep pockets. Knowing that smoking bans are a valuable weapon in the war against smoking, and knowing that ban implementations rest upon the fear that has been fanned around even slight smoke exposures, they have been all too willing to jump on the tax- and pharmaceutical-funded money train of Antismoking research. While it is probably rare that data is actually "faked," the very design of a study can often indicate the likely results, and whatever the results, they can always be "interpreted" with some degree of spin.

To give one very brief example: The 1998 WHO research authored by Boffetta et al. was released with a WHO public relations blitz headlined: "DON'T LET THEM FOOL YOU! PASSIVE SMOKING *DOES* CAUSE CANCER." (2) For the general public, this was the conclusion to be swallowed and used to promote otherwise unacceptable levels of behavior regulation.

However, if one actually reads the study, one finds that there were no statistically significant results to justify that headline, and that indeed the ONLY statistically valid conclusion reached by the study was that children of smokers had a lung cancer rate 22% **LOWER** than children of nonsmokers! Despite the fact that this was the only statistically significant conclusion found in the study, the official Abstract glossed over it by characterizing the finding as "no association." (3) Imagine the headlines that would have been trumpeted around the world if the opposite conclusion had been reached!

There may well be some almost invisible health effects from long term exposure to secondary tobacco smoke at higher than usual levels. All that the research so far has shown is that if such effects DO exist, they are incredibly small in terms of the decisions or concerns in any individual's life, particularly in situations with well-designed ventilation and high rates of air exchange. Fear, division, and even hatred have been encouraged by Antismoking groups seeking support for total smoking bans regardless of any such mitigating factors. It's a sad day for all of us when such forces reach into the world of basic science and health research and corrupt the scientific method.

- Michael J. McFadden

(1) www.forces.org/research/files/nci.htm

(2) www.who.int/inf-pr-1998/en/pr98-29.html

(3) tobacco.who.int/repository/tpc57/IARC.pdf

As noted below, no competing financial interests, however those who are interested may find me in ANR's "Tobacco Industry Tracking Database" because I had the temerity to criticize the EPA in a Letter To The Editor. A private email of mine to a smokers rights group (smokersrightsrjr) has been made public on the web as well, courtesy of The American Legacy Foundation, and may be found on their site.

Competing interests: I have no financial competing interests of any sort other than as a possible future author. I am a member of several smokers rights organization and have researched and written on the subject extensively.

Re: Misleading the public about secondhand smoke ... Again

27 May 2003

Wiel M Maessen,
Board member of Forces International
Netherlands

Send response to journal:
Re: Re: Misleading the public about secondhand smoke ... Again

Email Wiel M Maessen:
mailto:wiel@forces-nl.org?subject=Re:+Re:+Misleading+the+public+about+secondhand+smoke+...+Again

"If you want to know the interests, follow the money"

It's not the body of the message of Stanton Glantz, c.s. that is the most interesting, but the overview of competing interests that supplies a good view on the interests of these crusaders of the anti-smoking lobby. Thank you for the comprehensive overview of organisations that feed your fire. We will follow all the money and see what it really comes from.

The message they sent was very predictable, although one may wonder why they don't explain why one type of industry on the nicotine market is allowed to fund junk science and the other nicotine supplier isn't. We are sure that a major part of the funding comes from te pharmaceutical player on the nicotine market.

All three authors mention RWJF as one of their funders. What percentage of all their research was financed by this organisation?

Was it 10% or was it 90%? According to the online RWJF database[1], the University of California received $12,361,657 for tobacco

related research in the last 3 years. Stanton Glantz also mentioned the American Cancer Society as a source for grants. But also this organisation recently received $16,417,844 of grants from RWJF. The AHA also benefited from RWJF money: $2,811,670. How much of all this money was passed to Glantz's group?

Are these researchers hooked to pharma money? Do they serve the interests of the makers of nicotine patches and chewing gum? How much of this suspect research has been published by the Medical Journals?

What should we believe of their nicotine addiction theory when it's obviously a product of the ones who payed for their analyses?

Aren't we just talking about a habit here as was analysed by two scientists of Tel Aviv University

[2]? Isn't the persecution of

smokers and the ETS fraud a marketing strategy to push smokers in the hands of the pharmaceutical nicotine industry? Over and over again, because the theory is a hoax and the NRT products are little effective?

Please explain your own behaviour and interests, Mr. Glantz, before you point your finger to a study like this! We are desperately waiting for a comprehensive overview of all the money you are using to feed smoker persecution....

---

[1] http://www.rwjf.org/search/search.jsp?searchchoice=3 [2] A Critique of Nicotine Addiction, Reuven Dar, Hanan Frenk (http://www.amazon.co.uk/exec/obidos/ASIN/0792372255/forcesnederla-21/026- 6347872-6495619)

Competing interests: None declared

Re: Response to McKee and Diethelm 27 May 2003

Malcolm X. McGarrity,
Private Practice
Rancho Cucamonga 91730

Send response to journal:
Re: Re: Response to McKee and Diethelm

Email Malcolm X. McGarrity:
mailto:madmax@revolutionist.com?subject=Re:+Re:+Response+to+McKee+and+Diethelm

Dr. Kabat said: ""To imply that skepticism about the "weak association" of passive smoking with heart disease and lung cancer is due to influence from the tobacco industry is simply wrong-headed. There is legitimate debate about the effects of passive smoking on heart disease and lung cancer. The evidence is not as uniform or as strong as the activists and scientists with extra-scientific agendas make out. Interestingly, I have never before had this charge made against any of the work I have published on passive smoking or any other topic."

Never before has a special-interest group of social engineers had so much wealth and power in our society.

Awhile back Dr. Michael Siegel of the Boston University School of Public Health wrote an article titled "Responding to Tobacco Industry Attacks on the Scientific Evidence Linking Secondhand Smoke to Disease and Death" which was posted on the Americans for Nonsmokers Rights web page. In it he advises anti-tobacco activists "Do not get into arguments with the industry about scientific evidence... Instead, the best approach is to expose the tobacco industry ties of the so-called scientists making the arguments."

[This entire sordid story is memorialized on Steven Milloy's Junk Science website here: http://www.junkscience.com/sep99/anrorig.htm Milloy will no doubt be attacked as a tool of Big Tobacco for uncovering it and making it available, but links to the actual correspondence between the parties involved leave little room for denial.]

In enumerating scientists allegedly on the payroll of the tobacco industry, Siegel accused Robert

Levy and Rosalind Marimont of having "strong connections to the tobacco industry" due to their report, issued by the CATO Institute, attacking the CDC's estimate of 400,000 smoking deaths a year.

In response to Siegel's allegation, Levy challenged Siegel to provide supporting documentation, and in lieu of that, to issue a retraction and an apology.

Siegel indicated he had asked ANR to post a retraction and apology which Levy accepted provided it was posted on the ANR website.

However, ANR refused to post Siegel's retraction and apology stating, "After further discussion... [and] input from other [ANR] Board members, we have concluded that the possible 'clarification' that you and I discussed is simply not feasible...at this point ANR must put its political credibility ahead of what you consider to be your scientific credibility."

The crusaders obviously took Siegel's advice to heart as evidenced by the inane clamor over this study.

Thank you from those of us who believe the religion of "public health" is every bit as dangerous as tobacco use.

Malcolm X. McGarrity
Rancho Cucamonga 91730

Competing interests: None declared

Re: An American Cancer Society Perspective 29 May 2003

Daniel Forrest,
Private citizen
Brooklyn, NY 11234

Send response to journal:
Re: Re: An American Cancer Society Perspective

Email Daniel Forrest:
mailto:dkforrest@hotmail.com?subject=Re:+Re:+An+American+Cancer+Society+Perspective

"In other words, the findings of this study do not even differ significantly with the IARC estimates."

Just so. Two extensive studies, neither of which finds statistically significant harm from environmental tobacco smoke. And these two huge studies agree with 80% of all others.

When will you and other anti-tobacco crusaders admit there is no elephant in the refrigerator?

More importantly, when will the REST of the scientific community DEMAND truth and accuracy from you?

Competing interests: None declared

Re: Do any of the "non-smokers" smoke? 29 May 2003

John H. Glaser,
Independent researcher
4 Woodpark Circle, Lexington, MA 02421, USA

Send response to journal:
Re: Re: Do any of the "non-smokers" smoke?

Email John H. Glaser:
mailto:glaserj@alum.mit.edu?subject=Re:+Re:+Do+any+of+the+"non-smokers"+smoke?

I have been asked (via E-mail, from a visitor to this bmj.com site) whether the inverse correlation of smoking vs. education level was also true 20 to 30 years ago.

The answer is "yes." The fifteenth edition (1977-1979) of the Encyclopedia Britannica states that, "persons in the lowest occupational levels tend to start to smoke earlier and in greater numbers than those in higher levels."

Competing interests: None declared

Re: Re: Re: Inverse effect can be explained 29 May 2003

Adam Jacobs,
Director
Dianthus Medical Limited, London SW19 3TZ

Send response to journal:
Re: Re: Re: Re: Inverse effect can be explained

Email Adam Jacobs:
mailto:ajacobs@dianthus.co.uk?subject=Re:+Re:+Re:+Re:+Inverse+effect+can+be+explained

And is it also a narrow interpretation of the hygiene hypothesis to question the assumption that coronary heart disease has an allergic basis?

Incidentally, the paper that Maessen quotes in support of the idea that smoking can protect against allergy is a cross sectional study. Its results (a finding of lower prevalence of asthma among smokers) are therefore much more plausibly explained by a reluctance of asthmatics to do any further damage to their lungs by smoking than by suggesting that smoking protects against asthma.

Competing interests: None declared

Re: Defining a set of difficult issues 29 May 2003

Beverly A. HARRIS,
Pharmacy Manager
Shoppers Drug Mart V3J 3R3

Send response to journal:
Re: Re: Defining a set of difficult issues

Email Beverly A. HARRIS:
mailto:bh@lightspeed.ca?subject=Re:+Re:+Defining+a+set+of+difficult+issues

With all due respect to Lancet editor Richard Horton, unless his tongue was planted firmly in cheek

(I don't think that's the case), he's extremely naive, posing seven questions that are -- to anyone who knows anything about tobacco (and, more specifically, the nicotine cartel) -- not at all challenging.

For quick and easy reference, I'll number his questions....and my corresponding answers below.

In "5 (i)", Horton asks, 1) What if the tobacco industry (TI) sponsored work -- and good work too -- that showed that passive smoking was associated with higher levels of coronary disease? 2) Would this be acceptable for consideration by a medical journal? 3) Or should all tobacco-sponsored reasearch be banned from medical journals?"

Then, in "5 (ii)", Horton asks, 4) What if the TI sponsored work that had nothing to do with tobacco -- e.g., the safety of playgrounds (one example given to me)? 5) Would that work be acceptable? 6) Or would it, irrespective of the benefit it could bring to children's safety, be unacceptable because of the nature of the tobacco business? 7) What is it exactly that we are objecting to when we react against TI-sponsored work?"

All of the questions are based on an apparent assumption that the TI is not significantly different than any other 'good corporate citizen' industry. Again, anyone who knows anything about the industry and/or its products knows that tobacco is unique: IT'S THE ONLY LEGAL PRODUCT WHICH, WHEN USED EXACTLY AS INTENDED BY THE MANUFACTURER, PREMATURELY KILLS PEOPLE AT THE RATE OF ABOUT 3 MILLION PER YEAR, WORLDWIDE! And the industry's very survival depends almost exclusively on its ability hook and addict generation after generation of illegal/underaged kids! How many people do you know who started smoking after the age of 19....the minimum age for the purchase of tobacco in Canada?

1) I would suggest that Horton watch the movie, "The Insider", the real-life story of former-Brown & Williamson-research-executive-turned -whistleblower Dr. Jeffrey Wigand. The movie very clearly depicts just one small example of what happens to individuals within the nicotine cartel whose research reveals anything other than the PRE-DETERMINED -- and TOBACCO-FRIENDLY -- outcome. Last I heard, CBS Television was still providing round-the-clock security for Wigand.

2. Given what we know about the industry, no.

3. Yes.

4. Again, the question reflects an assumption the the TI is just another "good corporate citizen." If you see a tobacco executive hanging around a "playground", believe me, it has nothing whatsoever to do with "safety"....s/he is there for the exact same reason as every other drug dealer!

5. No.

6. Now we're catching on!

7. Again, given the TI's track record of deception and corruption, our reaction to industry-sponsored research is really quite natural/normal.

Sincerely,

Beverly A Harris

Competing interests: None declared

Cohort bias in the analysis of Californian passive smoking

29 May 2003

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Send response to journal:
Re: Cohort bias in the analysis of Californian passive smoking

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In Enstrom and Kabat's paper on passive smoking in California (1) there is a striking and perverse relationship between the level of spousal smoking in 1959 and the risk of death from Coronary Heart Disease (CHD) in the 'passive smoker' as defined by the study – the greater had been a man's cigarette consumption in 1959 the less likely, it seems, was the death of his wife from CHD.

I would suggest that there is a clear reason for this anomaly that illustrates a profound flaw in the paper's method and throws into question its findings.

Selection bias If we consider the group of never-smoking women married to smokers in the Californian paper, it is clear that there was a bias in their age distribution at the outset of the study. The 1959 mean age of women married to smokers decreased with the level of smoking in their spouses. Thus the wives of 40-a-day smokers had a mean age 4 years younger than the wives of 1 to 19-a-day smokers.

The emergence of this pattern is not surprising. Smokers are likely to die prematurely and this tendency increases with the heaviness of the habit. As a result, random selection from the general population will produce the observed bias in age of couples (assuming, reasonably, that there is an association between the age of spouses).

Never-smoking husbands of smoking women did not have the same age gradient, perhaps because of the small numbers (45) in the 40-a-day group. However, they had an average age in 1959 some 4-5 years lower than never- smoking husbands of never-smoking wives.

Taking the example of the wives of 40-a-day smokers, by virtue of being nearly 4 years younger on average than the control group, their age specific mortality rates, were one to calculate them, would be drawn from observations made on average 4 years later than the controls.

Cohort effect Against a background of stable mortality this might not matter. However, during the period of study, the CHD mortality rate in the USA, as in other western nations, was falling very rapidly (2) (http://www.cdc.gov/nchs/hus.htm). Between the mid-1960s and mid-1990s it fell by as much as two thirds in the 45-74 age group. On average this means that mortality rates for these age groups were falling by about 15% in every four year period.

Some of this change, perhaps as much as a third, may be attributed to changes in smoking behaviour (3) . The majority is attributable to other factors.

For any given age the 'passive smokers' were, thus, predominantly from a population whose overall CHD mortality had fallen significantly in relation to the controls of the same age.

In their analysis, Enstrom and Kabat used the Cox proportional hazard model. This approach, being a form of survival analysis, maps survival curves for compared groups from entry to the study and

tests for a difference between these curves.

Given the trend of rapidly changing societal CHD mortality, the mortality profile of a risk group is characteristic of an interaction between age and the time at which the observations were made, and is not simply a function of the age of the subjects within that risk group.

Adjusting for age alone will not remove the interaction of age and time of observation from the analysis. As a result, this cohort effect has not been controlled out of the calculations through age-adjustment. Indeed, no mention is made of it as a potential issue.

This cohort effect means that even if no difference in risk existed between female partners of 40-a-day smokers and controls, the partners of smokers should, in the absence of correction, exhibit a marked reduction in CHD mortality risk.

It is likely that this is the origin of the trend in relative risk seen in never-smoking women married to smoking men.

The spouses of smoking wives, on the other hand, are described in the results as having relative risks which, although non-significant, are below 1 for those married to smokers consuming 0-20 cigarettes and higher than 1 in the >21-a-day group. In this instance there is an apparent upward gradient with increased spousal smoking.

I would suggest in this instance that the lack of an age gradient at entry to the study may have allowed the emergence of a genuine gradient in association of CHD risk with passive smoking. The numbers are much smaller, but again it is likely that the relative risks have been diminished by a cohort effect. A real association may have been reduced in scale and rendered non-significant.

Other factors Davey Smith (4) has drawn attention to potential problems of confounding in Enstrom and Kabat's study. He and others have argued that being married to a smoker was a very poor indicator of passive smoking exposure for much of the period of this study, given the high degree of environmental exposure that obtained outside the home in past decades.

If the risk arising from passive smoking were an increase in CHD mortality of 30%, the latter factor alone would be likely to render smaller the observed difference. This issue has been explored elsewhere and I wouldn't wish to dwell on it further.

Further comment, however, should be made on use of the Cox proportional hazard model. This method is critically dependent upon proportionality between the two curves at all points following entry to the study (5).

It is not impossible to test for proportionality of hazard but it is difficult and is frequently not done. We are not told if such a test was performed by Enstrom & Kabat, but as it is not mentioned in their method, or in the statistical referees comments it seems likely that proportionality has been assumed.

Age-specific mortality from CHD increases exponentially in adult life other than at extremes of old age. In comparing two CHD survival curves for different risk groups, one is almost certainly comparing two exponential curves which differ not only in scale but also in doubling time. As such they cannot be proportional.

How much this non-proportionality matters is hard to say, but one would have to question the effectiveness of age-adjustment for two such curves under the Cox proportional hazard model. The curves are likely to differ in shape, both as a result of the cohort from which they are drawn and arising from any genuine difference in risk.

Conclusion The cohort effect described in this letter is potentially as large or larger than any of these factors. Although we cannot know with precision, its scale is plausibly sufficient to obliterate a genuine effect of passive smoking.

In its support: the effect can be seen to arise systematically through a random process; there is clear evidence of its existence in the presented data; the outcomes of analysis are consistent with its effects and their biologically implausible anomalies become explicable; allowance was not made for it in the method (nor identified as a problem in the referees' comments); and it would reconcile observations from this study with others.

Against this hypothesis: my assumptions are based on trends whose significance has not been statistically tested; and I have interpolated broad population trends to a specific population.

On balance, and taken together with the other flaws in the design, I would suggest that a cohort bias is a central flaw of the study of ETS and tobacco related mortality.

Importantly, the strength of this cohort effect is likely to be sufficient that the risk of passive smoking could quite plausibly be as high as previous studies have suggested, and yet still not be apparent under Enstrom and Kabat's method.

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Why the study? 30 May 2003

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I have read with interest the article and the many comments of the study conducted by Drs. Enstrom and Kabat. I am not familiar with Dr. Enstrom but had worked with Dr. Kabat for many years. He is a meticulous scientist and his analyses of the data are professional. Others have already pointed out many of the study limitations, but there are some issues that deserve further comment. Their suggestion that previous meta- analyses were problematic is incorrect. Meta-

analysis is conducted not just to derive a summary risk estimate, but to identify potentially flawed studies by testing heterogeneity between study designs, populations, and exposure assessment methods. The consistency of the positive findings across different study designs in published meta-analyses support a causal association. It was also suggested that the positive findings are due to publication bias but no evidence is offered. It is unfortunate that their conclusion in the text (not the abstract) regarding "environmental tobacco smoke and coronary heart disease and lung cancer" did not distinguish mortality from incidence. The very highly educated CPS I cohort members were likely to have sought high quality medical care to treat their conditions. Most other studies were based on incidence data and it might not be surprising that ETS is associated with disease incidence but not mortality. I doubt the failure to acknowledge this important subtlety was done intentionally but it lends credence to those who doubt the integrity of the authors rather than the reasoning behind their conclusions.

The big question is why is this paper worthy of publication regardless of its' scientific merit and the funding source? Even if the study was free from bias, the only conclusion that could be reached is that ETS was not a cause of mortality in the CPS II cohort. This finding alone does not change the weight-of-the-evidence, will not prompt further research in this area, will not overturn existing smoking bans in public places, and has no bearing on smoke avoidance since ETS contributes to other diseases such as asthma. Thus while equal opportunity should be welcomed from different perspectives, the editors failed to make a case that this study advances science or public health priorities.

Competing interests: None declared

Passive smoking: Why all the fuss? 30 May 2003

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Many of the respondents to the Enstrom and Kabat paper appear to be under a misapprehension. They seem to believe that it is an exception, an aberration which contradicts most similar studies.That is not so. When I reviewed passive smoking and lung cancer (Johnstone, 1991), the great majority showed no association. That is still the case.(Hackshaw et al, 1997)

It is apparent that many of your correspondents are opposed to anyone who is not anti-tobacco. It is not enough merely to be neutral. We are witnessing a new McCarthyism when anyone who dares say smoking is not or may not be harmful is an enemy of the people. Science has, for many commentators, long since departed and been replaced by fanaticism. The editor of "The Lancet" provides an unfortunate example when he equates tobacco and cluster bombs. Risk is a relative thing. A piece of beef steak can be a killer if swallowed imprudently. Faced with the choice between a bomb or a cigarette in his office I think he would make a life-preserving decision.

The Enstrom and Kabat paper does have a serious fault which has gone unnoticed: It is based on work done for the American Cancer Society by Hammond (1966). This was meant to compare the health of a large group of smokers as compared with a supposedly similar group of non-smokers. Hammond's early results were condemned when they first appeared (Berkson,1955, quoted in Wallis and Roberts,1962). It was clear that the two groups had been selected not at random but were heavily biased. Hammond himself admitted this error (Hammond and Horn, 1958). His work

suffered the ignominious fate of ending up as literally a textbook example of how not to conduct such a study (Wallis and Roberts,1962);the relevant pages can be found at:
http://members.iinet.net.au/~ray/hammond1.gif

http://members.iinet.net.au/~ray/hammond2.gif

Now,years later, this fatally flawed work has been revived, with no mention of its serious errors, to calculate deaths supposedly caused by tobacco. (Richmond, 1979; Holman et al, 1990). It should be compared with another study (summarised at http://members.iinet.net.au/~ray/ABS43820d.jpg) where the subjects were chosen at random by statisticians rather than selectively by volunteers from the American Cancer Society (Castles,1992) and which found smokers less likely to suffer from most long-term conditions including cancer, hypertension and heart disease.

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